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CONDUCTED BY  
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“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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VOL. XXXIX.—FOURTH SERIES.  
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“Meditationis est perscrutari occulta; contemplationis est admirari  
perspicua . . . . Admiratio generat quæstionem, quæstio investigationem,  
investigatio inventionem.”—*Hugo de S. Victore.*

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—“Cur spirent venti, cur terra dehiscat,  
Cur mare turgescat, pelago cur tantus amaror,  
Cur caput obscura Phœbus ferrugine condât,  
Quid toties diros cogat flagrare cometas;  
Quid pariat nubes, veniant cur fulmina cœlo,  
Quo micet igne Iris, superos quis conciat orbes  
Tam vario motu.”

*J. B. Pinelli ad Mazonium.*

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# CONTENTS OF VOL. XXXIX.

## (FOURTH SERIES.)

### NUMBER CCLVIII.—JANUARY 1870.

	Page
Canon Moseley on the Mechanical Properties of Ice. (With a Plate.) . . . . .	1
Mr. R. Templeton's Remarks suggested by Mr. Douglas's Account of a New Optometer . . . . .	9
Mr. J. A. Phillips's Note on certain Slates, Felsites, and Elvanites occurring near Knockmahon in the County of Waterford. . . . .	12
Mr. G. K. Winter's observations of the Corona during the Total Eclipse, August 7, 1869. . . . .	17
Prof. Challis on a Mathematical Theory of Tides . . . . .	18
Mr. C. Tomlinson on the Motions of certain Liquids on the Surface of Water . . . . .	32
The Rev. T. K. Abbott's Note on some Propositions in the Theory of the Tides . . . . .	49
Mr. J. Parnell on Secondary Currents and a new Secondary Battery . . . . .	52
Mr. J. N. Lockyer's Reply to some Remarks of Father Secchi on the recent Solar Discoveries . . . . .	61
Proceedings of the Royal Society:—	
Mr. H. C. Sorby on Jargonium, a new Elementary Substance associated with Zirconium . . . . .	65
Mr. J. P. Harrison on Solar Radiation . . . . .	70
On the Laws of Induction. By MM. Jamin and Roger . . . .	73
On the Calorific Power of the Lunar Rays, by M. H. Marie-Davy. . . . .	78

### NUMBER CCLIX.—FEBRUARY.

Mr. J. Croll on Ocean-currents.—Part I. Ocean-currents in relation to the Distribution of Heat over the Globe. . . . .	81
Prof. R. S. Ball's Note on an Elementary Proof of a Theorem of Lagrange's . . . . .	107
Mr. G. K. Winter on the determination of the size of the Wire which, wound upon a Galvanometer or Electromagnet, will produce the maximum magnetic effect in a Circuit of given external resistance. (With a Plate.) . . . . .	109

M. F. Kohlrausch's Determination of the Absolute Horizontal Intensity of the Earth's Magnetism by Measurement of a Current .....	114
Mr. W. H. Walenn's Remarks on the Atomic Theory .....	122
Prof. W. A. Norton on the Fundamental Principles of Molecular Physics .....	126
M. V. Regnault's Memoir on the Expansion of Gases .....	127
Proceedings of the Royal Society :—	
Dr. H. E. Roscoe's Researches on Vanadium.—Part II. . .	146
Dr. T. Andrews on the Continuity of the Gaseous and Liquid States of Matter .....	150
On the Simultaneous Occurrence of a Soluble Lead-salt and free Sulphuric Acid in Sherry Wine; with observations on the Solvent Action of Alcoholic Saline Solutions upon Sulphate of Lead, by Professor F. H. Storer .....	154
On Auroral Appearances and their connexion with the Phenomena of Terrestrial Magnetism, by Balfour Stewart, F.R.S., F.R.A.S. ....	159

## NUMBER CCLX.—MARCH.

Dr. E. Warburg on the Deadening of the Sounds of Solid Bodies by Internal Resistances. ....	161
Mr. T. T. P. B. Warren's Notes on Professor Fleeming Jenkin's Formula .....	169
Mr. E. L. Garbett on Popular Difficulties in Tide Theory ....	174
Mr. J. Croll on Ocean-currents.—Part II. Ocean-currents in relation to the Physical Theory of Secular Changes of Climate. ....	180
Prof. Reusch on Mica-combinations .....	195
Sir James Cockle on Criticoids .....	201
Dr. W. J. M. Rankine on the Thermal Energy of Molecular Vortices .....	211
M. L. Soret on Illumination and Polarization in Transparent Substances .....	221
M. A. De la Rive on the Organic Dust of the Air .....	229
Proceedings of the Royal Society :—	
Dr. J. H. Gladstone on the Refraction-Equivalents of the Elements .....	231
On Comets and Meteors, by Daniel Kirkwood, LL.D., Professor in Indiana University .....	233
On a possible Cause of the Gulf-stream, by Robert Gill, Esq. ....	238

## NUMBER CCLXI.—APRIL.

Canon Moseley on the "Veined Structure" of the Ice of Glaciers. (With a Plate.) .....	241
---	-----

	Page
Mr. R. Moon on the Structure of the Human Ear, and on the Mode in which it administers to the Perception of Sound ..	248
Prof. Challis on a new discussion of the Mathematical Theory of Oceanic Tides .....	260
Prof. How's Contributions to the Mineralogy of Nova Scotia ..	275
Prof. Tyndall on the Polarization of Heat .....	280
Prof. F. Guthrie on $\sqrt{-1}$ .....	282
Mr. J. A. Wanklyn on the Action of the Alkali-metals on the Ethers of the Fatty Acids .....	286
The Rev. J. M. Heath on the Circumstances which determine the Variation of Temperature in a Perfect Gas during Expansion and Condensation .....	288
Messrs. B. Silliman and H. Wurtz's Investigation of Flame-temperatures, in their Relations to Composition and Luminosity.—First Memoir .....	290
Mr. J. P. Harrison on Solar Radiation in connexion with Cloud and Vapour .....	299
Notices respecting New Books :—	
Mr. F. Edwards's Our Domestic Fireplaces .....	300
Dr. A. M. Mayer's Lecture-notes on Physics .....	302
Mr. E. Dexter's Theory of Existence .....	302
Proceedings of the Royal Society :—	
Mr. W. H. L. Russell on the Mechanical Description of Curves .....	304
Prof. W. J. M. Rankine on the Thermodynamic Theory of Waves of Finite Longitudinal Disturbance .....	306
Mr. F. Guthrie on Approach caused by Vibration .....	309
Messrs. E. Frankland and B. F. Duppa on the successive Action of Sodium and Iodide of Ethyle upon Acetic Ether.	309
Proceedings of the Geological Society :—	
Mr. G. W. Ormerod on the Structure of the Granite on the Eastern side of Dartmoor .....	313
Mr. D. Mackintosh on Lithodomous Perforations on the Hills of North-west Lancashire .....	313
Prof. J. Nicol on the Parallel Roads of Glen Roy .....	314
Mr. J. C. Ward on Beds of supposed "Rothliegende" age near Knaresborough .....	314
Researches on Solar Radiation, by MM. P. Desains and E. Branley .....	315
Avogadro's Law deduced from the Fundamental Conception of the Mechanical Theory of Gases, by Alex. Naumann .....	317

---

NUMBER CCLXII.—MAY.

M. F. J. Pictet on the present state of the question as to the Limits of the Jurassic and Cretaceous Periods .....	321
Prof. J. C. Draper on an Improvement in Filtration .....	335



	Page
Dr. W. M. Watts's Note on the Temperature and Heating-powers of Flames .....	337
Mr. J. Dewar's Note on the Atomic Volume of Solid Substances.	339
Mr. J. Dewar's Note on Inverted Sugar .....	345
The Rev. J. M. Heath on the Theory of the Variation of Temperature in Gases in consequence of Changes in their Density and Pressure .....	347
M. H. Kolbe on the Province of Mineral Chemistry .....	348
Captain A. R. Clarke on the Course of Geodesic Lines on the Earth's Surface .....	352
Mr. T. S. Aldis on Goodricke's Theory of Algol .....	363
M. A. Willner on the Spectra of some Gases under High Pressures .....	365
Mr. J. A. Calantarients's Description of a New portable Mercurial Barometer .....	371
Proceedings of the Royal Society:—	
Mr. G. Gore on Fluoride of Silver.—Part I. ....	374
Mr. E. J. Stone on the Heating-powers of Arcturus and $\alpha$ Lyrae .....	376
Mr. R. A. Proctor on certain Drifting Motions of the Stars.	381
Proceedings of the Geological Society:—	
Dr. A. Rattray on the Geology of Cape-York Peninsula, Australia .....	383
Messrs. H. W. Bristow and W. Whitaker on the Formation of the Chesil Bank, Dorset.....	384
Mr. W. Whitaker on a Raised Beach at Portland Bill, Dorset .....	384
Messrs. E. Tawney and T. Davidson on the occurrence of <i>Terebratula diphya</i> in the Alps of the Canton de Vaud..	385
Prof. Huxley and Mr. L. C. Miall on a new Labyrinthodont from Bradford .....	385
Prof. Huxley on the Maxilla of <i>Megalosaurus</i> .....	385
Dr. G. H. F. Ulrich on the 'Nuggetty Reef,' Mount Tar-rangower Gold-field .....	386
Dr. C. Le Neve Foster on the Caratal Gold-field .....	386
Mr. R. Tate on the Geology of Guyana in Venezuela ..	386
Mr. J. J. Murphy on the Nature and Cause of the Glacial Climate .....	387
Sir Philip Grey Egerton on two new Species of <i>Gyrodus</i> ..	387
Mr. J. W. Hulke on a Saurian Humerus from the Kimmeridge Clay of the Dorset coast, and on some Fossil remains of a Gavial-like Saurian from Kimmeridge Bay..	388
Mr. W. T. Blanford on the Geology of a Portion of Abyssinia .....	389
Prof. J. W. Dawson on the Graphite of the Laurentian of Canada .....	389
Mr. D. Mackintosh on the Correlation, Nature, and Origin of the Drifts of North-west Lancashire and part of Cumberland ..	389

	Page
Mr. W. Whitaker on the Connexion of the Geological Structure and Physical Features of the South-east of England with the Consumption Death-rate .....	390
The Rev. C. G. Williamson on the Volcanic Phenomena of Hawaii .....	390
Dr. H. A. Nicholson on certain of the Intrusive Igneous Rocks of the Lake-district .....	391
Mr. S. H. Scudder on the Fossil Myriopods of the Coal-formation of Nova Scotia and England .....	391
Mr. A. Rogers on the Geology of the Country surrounding the Gulf of Cambay .....	391
Mr. J. W. Mason on a new Acrodont Saurian from the Lower Chalk .....	391
Mr. W. A. Sanford on Rodentia of the Somerset Caves ..	392
On a new kind of Electrical Dust-figures, by Professor von Bezold .....	392
Action of Sodium on Acetic Ether .....	394
On the Periods of certain Meteoric Rings, by Daniel Kirkwood.	394
On the Diathermancy of a Series of Substances for Obscure Heat, by Dr. Schultz-Sellack .....	396
On the Influence of Sounding-vibrations on the Magnetism of Iron, by Dr. E. Warburg .....	398

---

### NUMBER CCLXIII.—JUNE.

Mr. A. S. Davis on a Theory of Nebulæ and Comets .....	401
M. J. Thomsen's Thermo-chemical Researches .....	410
The Rev. J. M. Heath on Thermodynamics .....	421
Mr. J. L. Tupper on an Optical Illusion .....	423
The Hon. J. W. Strutt on an Electromagnetic Experiment ..	428
The Rev. Prof. Challis's Supplement to a Mathematical Theory of Oceanic Tides .....	435
M. F. L. Ekman on the Connexion between the Electrical Insulating-power and Chemical Composition of various kinds of Glass .....	437
The late Prof. G. Magnus on the Change in the Radiation of Heat by Roughness of the Surface .....	445
Proceedings of the Royal Society:—	
Mr. H. C. Sorby on some remarkable Spectra of Compounds of Zirconia and the Oxides of Uranium .....	450
Dr. W. J. M. Rankine on the Mathematical Theory of Stream-lines, especially with those four Foci and upwards .....	460
Proceedings of the Geological Society:—	
Mr. C. Moore on Australian Mesozoic Geology and Palæontology .....	462
Mr. C. Moore on a Plant- and Insect-bed on the Rocky-River, New South Wales .....	463

	Page
On the Inexpediency of Erroneous Hypotheses, by Charles Brooke .....	464
Results of a Research on the Absorption-spectrum of Iodine-vapour, by Robert Thalén.....	465
On a simple Method of dispensing with observations of Temperature and Pressure in Gas-analyses, by Wolcott Gibbs, M.D.	465
Physical Notices, by J. Müller .....	467
Index .....	468

## PLATES.

- I. Illustrative of Canon Moseley's Paper on the Mechanical Properties of Ice.
- II. Illustrative of Mr. G. K. Winter's Paper on the determination of the size of the Wire which, wound upon a Galvanometer or Electromagnet, will produce the maximum magnetic effect in a Circuit of given external resistance.
- III. Illustrative of Canon Moseley's Paper on the "Veined Structure" of the Ice of Glaciers.

## ERRATA.

- Page 55, note, line 3 from the bottom, *for observation read operation*  
 — 112, line 8 from bottom, *for equations (2) and (4) read (1) and (4)*  
 — 113, line 2, *for equation (2) or (4) read equation (1) or (4)*  
 — 113, column 3 of Table, *for equation (2) read equation (1)*

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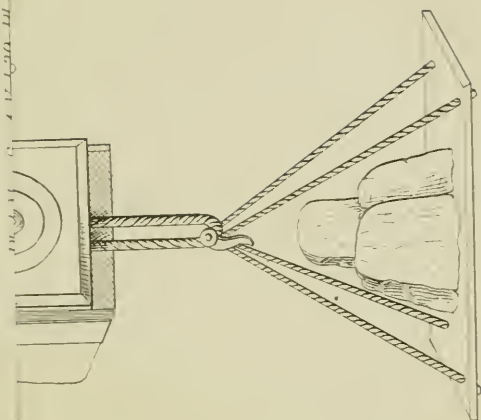
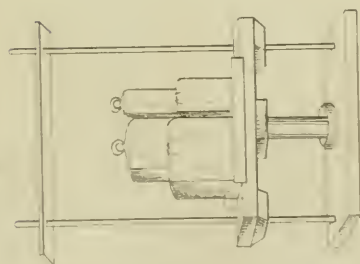


Fig 7



Vincent Brooks, Day & Son, Ill.





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[FOURTH SERIES.]

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JANUARY 1870.

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I. *On the Mechanical Properties of Ice.*  
By HENRY MOSELEY, F.R.S., Canon of Bristol\*.

[With a Plate.]

*The Dilatation of Ice.*

IN the winters of 1845 and 1846 three series of experiments on the dilatation of ice were made independently, by MM. C. Schumacher of Copenhagen, Moritz of Dorpat, and Pohrt at the Observatory of Pultowa. These experiments have been described by W. Struve in the memoirs of the Academy of St. Petersburg †.

The experiments of M. Moritz differed from the others in this, that he measured the dilatation in a direction perpendicular to the surface of the water out of which the ice was frozen, whilst the others measured it in a direction parallel to that surface.

The results of the three series of experiments were nearly identical.

The coefficient of linear dilatation for  $1^{\circ}$  C. was, according to M. C. Schumacher, between the temperatures  $-1^{\circ}$  and  $-22^{\circ}$  R.,  $\cdot 00006424$ , with a probable error of  $\cdot 00000019$ ; according to M. Pohrt, between  $-0^{\circ}9$  and  $-21^{\circ}5$  R.,  $\cdot 00006387$ , with a probable error of  $\cdot 00000046$ ; according to M. Moritz, between  $-1^{\circ}36$  and  $-22^{\circ}82$  R.,  $\cdot 00006469$ , with a probable error of  $\cdot 00000061$ . The mean of these three measures is  $\cdot 00006427$ , with a probable error of  $\cdot 00000018$ .

\* Communicated by the Author.

† *Sciences Mathém. et Phys.* S. 6. vol. iv.

For  $80^{\circ}$  R., or for the whole dilatation between the freezing and boiling temperatures, this gives the coefficient

$$\cdot 005142*.$$

And as there are  $180^{\circ}$  of Fahrenheit between these temperatures, the coefficient of the dilatation of ice for  $1^{\circ}$  F. is

$$\cdot 00002856.$$

The experiments of M. C. Schumacher were the most numerous and the most complete. They were conducted as follows.

By pouring water which had been previously boiled into a wooden trough or mould, and leaving it on trestles exposed to the air for twenty-four hours, it was frozen into a solid block of ice tolerably free from air-bubbles, which, when brought into a regular shape and rounded at its angles, was 6 feet 3 inches long,  $6\frac{1}{2}$  inches deep, and 6 inches wide. A longitudinal section of this block of ice is shown in Plate I. fig. 2 A, and a transverse one in fig. 3.

To have fixed points on the block from which to measure its dilatation, two steel bolts ( $b$  and  $b'$ ) were made to pierce the block from side to side near its ends, and were frozen into it. Each of these bolts had fixed across one end a silvered brass plate ( $n$ ,  $n'$ , fig. 2) with divisions; and the other end of each bolt was pierced in the direction of its axis by a cylindrical hole ( $p$ ,  $p'$ ). The two bolts had their divided plates on opposite sides of the block, and also the holes pierced in their extremities. In the upper surface of the block of ice three equidistant holes were bored ( $t'$ ,  $t''$ ,  $t'''$ ) halfway through its thickness, into each of which a thermometer was frozen. The bulb of each thermometer was contained in a cavity of the ice larger than itself, so as to be surrounded by air, all communication between which and the external air was cut off by the stem being frozen into the ice. The block of ice was then slung, at about 8 feet from the ground, by two cords passed round it near its ends, to a beam supported on trestles (fig. 1) which stood on one of the projections of the peristyle of the observatory, where it was exposed to the free influence of the external temperature.

To determine the different distances of the two bolts at different temperatures of the block of ice as shown by the thermometers, it was necessary to have, 1st, a standard measure not affected by variations of temperature, with which to compare these distances, and, 2ndly, some reliable means of comparing them with

\* Of other solids, the most dilatable are lead (whose coefficient of dilatation between the same limits of temperature is  $\cdot 002848$ ) and zinc (whose coefficient is  $\cdot 002976$ ); the dilatation of ice for a given increase of temperature is therefore nearly twice as great as that of any other known solid body.

that standard. For the standard measure, two bolts were driven into the wall of a room of the observatory, in the same horizontal line and at the same distance from one another as the bolts in the block of ice. One of these bolts had a hole sunk in its end in the direction of its axis, of the same size as the holes in the bolts inserted in the ice; and the other bolt carried at its end a divided plate similar to the plates on those bolts. The room, in the wall of which these bolts were fixed, was preserved as nearly as possible at the same temperature\*. To compare with the distance of these bolts the distances at different temperatures of the bolts in the block of ice, a rod (B, fig. 2) of dry white deal was used, 5 feet 3 inches long, having for its section a square whose sides were in the middle  $1\frac{2}{3}$  inch in length, but tapered off at the ends to 1 inch. On each end of this rod was firmly fixed a brass ring,  $a a'$ , 1.83 inch in diameter. Fixed to one of these rings, parallel to the axis of the rod and at the extremities of the same diameter of the ring, were two plates ( $m, m'$ ) of brass, whose surfaces were silvered, and divided on their chamfered edges, so as to be applied to the corresponding plates on the bolts in the ice-block.

Through the other ring, as also through the measuring-rod itself, was passed a bolt,  $s s'$ , which had at each extremity a pin of hardened steel accurately fitting the holes sunk in the ends of the bolts which passed through the ice†. The whole stick was thickly wound round with cotton-wool sewn up in linen cloth, so that it could only be very slowly affected by variations of external temperature. The divisions of the plates on the rod and on the ends of the bolts in the ice were so adjusted as to form a vernier measuring to the  $\frac{1}{1800}$  of an inch. When not in use the measuring-rod was hung on the wall the bolts in which served as the standard of measurement, so as to acquire its constant temperature. When used, the rod was carried out, and the pin passing through the ring at one of its ends was passed into the hole sunk in the extremity of the bolt projecting from one of the two vertical faces of the ice-block, the plate on the ring at the opposite end being at the same time brought into contact with the plate on the end of the other bolt which projected from the same face of the ice-block. The distance having been read off by the vernier, the rod was turned round on its axis  $180^\circ$ , so as to bring the opposite pin into the hole sunken in the one bolt in the ice, and the opposite plate into contact with the plate fixed on

\* It was situated between two other rooms, which were heated with hot air from the same stove, the access of which could be regulated.

† There was an ingenious contrivance by which the insertion of the same length of this pin into the hole of the bolt at each experiment was provided for.

the other bolt. The measurements being in each case taken, and the heights of the thermometers recorded, the measuring-rod was carried to the opposite vertical face of the block, and they were there repeated. Corrections were thus applied for the differences of expansion on opposite sides of the ice-block, and distortions in the form of the measuring-rod. Before the measuring-rod was brought out, at each observation, it was compared with the standard length on the wall, and when it was brought back the comparison was repeated.

The observations began on the 11th of February, and by the continuance of frost M. Schumacher was enabled to go on with them to the 24th of March. During this time he made 153 different admeasurements, each separate and complete in itself, but all on the same ice-block. All are included in the mean of his results.

When he had completed 72 observations, M. Schumacher determined to test the permanence of the temperature of the wall in which were fixed the two bolts whose distance was his standard of measure. For this purpose he bent the tube of a thermometer at a right angle three inches above the bulb, and made a corresponding hole in the wall, into which he plastered up the bent part of the thermometer, but so that the bulb should stand free of the masonry. The temperature of the wall was found to vary as much as  $3^{\circ}$  R.

It is not necessary to describe in detail the corrections made by M. Schumacher for the resulting variation in his standard; or those by which he corrected other errors incidental to his experiments, or the expedients he adopted in the construction of his thermometers, or the corrections he applied to his readings from them. They are models of ingenuity and laborious fidelity in observation. As the final result he gives

The coefficient of the linear expansion of ice for $1^{\circ}$ R. between the limits $-1^{\circ}\cdot6$ R. and $-21^{\circ}\cdot8$ R. ....	} ·000064236, with a probable error of ·000000193.
The coefficient of the influence of the difference of the temperature of the air from that of the ice-block per $1^{\circ}$ R. ...	
The coefficient of the expansion of the standard measure. ....	} ·000002165, with a probable error of ·000001064.
	} ·000006636, with a probable error of ·000001027.

#### *The rate of Cooling of Ice.*

To determine the rate of cooling of ice, M. Schumacher brought his block, when at a temperature of  $-2^{\circ}$  R, and hung it in the open air when at a temperature of  $-17^{\circ}$  R. He then observed the descent of its temperature by the readings of the thermometers frozen into it every five minutes. From these observations it resulted that the sinking ( $\Delta t$ ) of the temperature of the ice

in each successive five minutes equalled the mean difference  $(\tau - t)$  between the temperature of the air and that of the ice multiplied by  $\cdot 056$ , or

$$(\tau - t) \cdot 056 - \Delta t = 0.$$

### The Tenacity of Ice.

I caused a rod of ice to be cut out of a solid block and turned in a lathe (see fig. 4). It was 12 inches in length and had conical ends, from which its diameter tapered to  $1\frac{1}{2}$  inch in the middle of its length. To clasp the two ends of the rod so as to apply the strain required to tear the rod asunder, two pieces of wood hinged together (fig. 5) had a cavity cut in each, so that when they were shut and screwed together they enclosed one of the conical ends of the ice. Two such clasps were constructed; and each had an iron loop fixed to it, by which one end of the ice-cylinder could be hooked up and the other have weights suspended to it (fig. 6). The first set of experiments was made on the 26th of August, 1869—one of the hottest days of that year. The thermometer ranged in the room from  $70^\circ$  to  $75^\circ$ , and the ice was almost in a deliquescent state. The second set of experiments was made on the 27th of October, 1869. There are well known to be great differences in the hardness of different specimens of ice at the same temperature, and yet greater at different temperatures. To these correspond different tenacities.

### Experiments on the Tenacity of Ice.

Number of experiment.	Diameter at the point where the separation took place, in inches.	Strain which tore the ice asunder, in lbs.	Tenacity in lbs. per square inch.	
1.	$\frac{23}{16}$	120	73.94	{ Experiments made on the 26th of August, 1869. Thermometer $70-75^\circ$ .
2.	$\frac{11}{8}$	104	70.039	
3.	$\frac{19}{16}$	102	92.097	
4.	$\frac{89}{64}$	151	99.42	
5.	$\frac{3}{12}$	206 It bore this load 10', and then broke.	116.57	{ Experiments made on the 27th of October, 1869.
6.	$\frac{3}{12}$	193 Broke in 14'.	109.22	
7.	$\frac{3}{12}$	Bore 122 for 30'; then 84 were added, and it broke in 5'.	116.57	
8.	$\frac{3}{12}$	Bore 150 for 20'; then 56 were added, and it broke in 6'.	116.57	



*Experiments on the Crushing of Ice, August 26, 1869.*

A wooden frame was constructed (fig. 7), having fixed to its base two vertical iron rods, on which traversed a strong wooden platform intended to carry the weights necessary to crush a cylinder of ice placed vertically beneath the platform. The first experiment was made with a cylinder  $1\frac{1}{2}$  inch in diameter and  $2\frac{1}{16}$  inches long. It bore without crushing 578 lbs. for forty minutes, during which time its diameter diminished by thawing to  $1\frac{3}{8}$  inch. Planes of cleavage at right angles to the axis formed themselves as the loading went on, all down the cylinder, and also longitudinal planes, some of which passed through the axis, and others were parallel to it. This shorter cylinder was then replaced by one of the same diameter ( $1\frac{1}{2}$  inch), but 6 inches long. It showed signs of failure, first, by a slight bending in the middle, then by the formation of a plane of shearing at an inclination of  $23^\circ$  to the horizon at its middle point, round the edges of which plane it bulged out into a sort of lip (see fig. 10). Along this plane it slowly sheared with a pressure of 545 lbs. While it was in the act of shearing, planes of cleavage formed themselves parallel to the axis; and along them it finally split up. Knowing the inclination ( $\iota$ ) of the plane of shearing, the radius ( $r$ ) of the cylinder, the incumbent pressure  $P$ , and the limiting angle ( $\phi$ ) of the resistance of friction, the unit of shear ( $\mu$ ) is determined by the equation

$$\mu = \frac{P \sin (\iota - \phi) \cos \iota}{\pi r^2 \cos \phi};$$

from this formula (assuming  $\phi$  to be  $2^\circ$ ) we obtain

$$\mu = 101.8 \text{ lbs.}$$

This will be found not to differ much from the unit of shear determined by the direct shearing of ice. The crushing-pressure (545 lbs. upon a cylinder  $1\frac{1}{2}$  inch in diameter) is equivalent to 308.4 lbs. per square inch. Assuming ice to be of the same specific gravity as water, a strip 1 square inch in section and 710 feet high would have this weight. Supposing a great number of such strips to be placed together vertically so as to form a glacier, that glacier (710 feet deep) would only just, by its weight, crush itself upon its base.

As there is no glacier alleged to have so great a depth as 710 feet, this fact is an answer to that theory which attributes the descent of a glacier to the crushing of the ice of its base by the incumbent weight.

### *The Shearing of Ice.*

Two blocks of hard wood, each 3 inches in thickness, had their surfaces carefully fitted to one another, and the one was adjusted so as to slide on the other by means of guides. The two were then pierced through in a direction perpendicular to their planes of contact by a cylindrical hole  $1\frac{1}{2}$  inch in diameter. The apparatus (fig. 8) was used in a vertical position, one of the blocks being screwed to a carpenter's bench near the edge, the other block overhanging the edge. From this second moveable block a scale-pan was suspended by a strong cord. The two blocks being then brought so that the cylindrical hole in the one corresponded with that in the other, a cylinder of ice turned in the lathe to a diameter of  $1\frac{1}{2}$  inch was inserted through the two, and weights were added to the scale-pan until the cylinder of ice began to be sheared across. The process of shearing was slow, notwithstanding that it was accelerated by the melting of the ice by reason of the heat conducted from the apparatus, and of the pressure sustained at the upper and lower surfaces of the cylinder, by which its temperature of congelation there was lowered. After about thirty minutes the weights were removed, and the cylinder, now sheared partly across (fig. 9), was taken out of the apparatus. There was not the least appearance of separation in the plane *ab*, over which a surface of ice had been continually shearing. The new surfaces made to slide into contact had entered into precisely the same relations with one another as they had to the surfaces with which they were before in contact, and the ice was continuous.

Thirteen such experiments were made. Their results are recorded in the following Table\*. The last six were made, not with cylinders turned out of solid ice, but formed by regelation—by hammering with a mallet and a solid rammer broken pieces of ice into the cylindrical holes of the apparatus when brought together so as to form *one* hollow cylinder.

\* The unit of shear cannot be determined by experiment with absolute accuracy, except by taking into account the observed time of shearing. In the case of a cylinder this determination is difficult; but in the case of a prism having a rectangular section it is comparatively easy. If *a* and *b* be the sides of such a section in feet, *W* the shearing-weight in lbs.,  $\mu$  the unit of shear in lbs. per square foot, and *t* the time, in seconds, of shearing through the distance *x*,

$$t = \frac{W}{\mu b g} \log_e \left\{ 1 + \frac{\mu b x}{W - a b \mu} \right\},$$

where *a*, *b*, *g* are known, and *W*, *t*, *x* are determined by the experiment; the equation determines therefore  $\mu$ .

Experiments on the Shearing of Ice, August 24, 1869. Thermometer 70–75°. Diameter of cylinder sheared  $1\frac{1}{2}$  inch.

Shearing weight, in lbs.	Distance sheared*.	Time during which the shearing was allowed to go on, in minutes.	Unit of shear, in lbs. per square inch.	
180	inch.	32	101·86	{ The apparatus was hot from previous handling in these experiments; the ice sheared therefore more easily than afterwards.
173	$1\frac{1}{8}$	36	97·898	
180	$1\frac{1}{8}$	30	101·86	
202	$\frac{5}{8}$	30	114·31	
202	$\frac{5}{8}$	29	114·31	
196	$1\frac{1}{8}$	$30\frac{1}{2}$	110·91	
196	$\frac{5}{8}$	30	110·91	
202	$\frac{3}{4}$	18	114·31	{ In these experiments the cylinder of ice was not turned out of the solid, but formed by regelation.
202	$1\frac{3}{8}$	20	114·31	
180	$\frac{3}{4}$	20	101·86	
196	$\frac{5}{8}$	21	110·91	
210	.....	3	118·84	{ The load was in these experiments sufficient to shear the ice rapidly.
210	.....	2	118·84	

*The passage of Radiant Heat through Ice.*

That ice is diathermanous has been shown by others. I have verified it by the following experiment. Finding that ice could easily be turned in the lathe, I had an ice lens constructed by means of an iron templet whose edge was a circle 12 inches in radius. I thus obtained a lens whose two faces were surfaces of spheres of that radius, and whose focal distance was 19 inches. When the sun's rays were received on this lens, the light streamed through it in abundance, and it was not possible to hold the hand in its focus by reason of the heat. A lucifer-match placed there was instantly lighted. The experiment was repeated three times. Ice lenses would be so easily made and renewed that it would not perhaps be impossible to use them for astronomical purposes in countries the temperature of which is below that of freezing for two or three months of the year. They might be constructed of any size; and ice is to be found of different densities, and therefore probably of different refracting-indices.

\* This distance is that shown in fig. 10 by A a or b B.

II. *Remarks suggested by Mr. Douglas's Account of a new Optometer.* By R. TEMPLETON, Esq.\*

IN the Number of the Philosophical Magazine for May, 1869, a notice appeared of an optometer, considered by the author an improvement on that described by Dr. Donders. No exact measurements being given, I am not quite certain that I exactly realize the description given by Mr. Douglas; but presuming that I do, I think I can satisfy him that the indications of such an instrument are fallacious.

It is assumed, then, that a little screen (2 millims. in diameter) is interposed betwixt the eye and a source of light about 250 millims. distant from it: moving this source of light nearer to, or further from the eye, a certain distance is determined, at which, as I understand Mr. Douglas, the two pencils of light, one passing on either side of the screen, cease to be separated from each other, one impression only being received upon the retina.

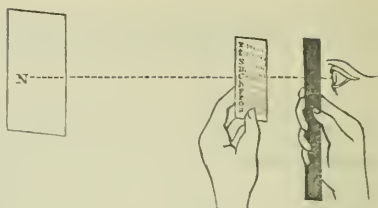
To test this, let two small screens be used, one the largest possible that the pupil will admit of using, the other of smaller dimensions; and let exact measurements be made of their breadth, and of the distances of the screens and of the source of light, at the moment preceding the perception of the single image. If now, to fix the ideas, a large horizontal section of the eye be laid down upon paper, using the data given in Dr. Donders's and other recent works, and these pencils be traced upon it to their termination on the arch representing the first surface of the retina, it will be found that they do not meet at that surface. No reasonable hypothesis can make them do so.

The case is simply an extreme example of the experiment described half a century ago in Brewster's Journal: the looking at a pin in a bright light through a series of minute holes (fifteen to the centimetre) in a thin sheet of brass; six or eight images of the pin are seen, which are true images imprinted on the retina, as can be easily shown.

Next, let a leaf of paper be taken, on which is good-sized and distinct printing, and let it be doubled up so that a line of letters may be close to the edge of the fold: let this paper so folded be brought to a shorter distance than that of clear vision, the letters becoming misty and confused; now move leisurely across the paper, but much nearer to the eye, a somewhat dark-coloured ruler, approaching it gradually to the folded edge: close in along the edge of the ruler the letters will become distinctly visible

\* Communicated by the Author.

and sharply defined. Let now a large capital letter be pinned up against the window-shutter frame (so as to have the advantage of a good light), and bring (see figure) the eye carefully up to it to the distance of ordinary correct vision; then



slip in between the eye and the capital letter the folded-up paper with the ruler, so that the letters along its edge, distinctly seen, may be observed in the line of the capital letter: the smaller letters and the capital letter can now be seen at the self-same moment with perfect distinctness. The result is very striking, and, I need scarcely add, goes to the root of all hypotheses of lens adjustment\*.

Physicists have too readily, and on quite insufficient evidence, adopted the opinion that the eye is simply a camera, with a lens to form a picture, a retinal surface to receive a picture, and an adjusting-power for distance. Since the date of my paper, in vol. xxxii. S. 4. of *Phil. Mag.* p. 425†, I have seized every

\* I am acquainted with a case of reticular cataract in which three of the meshes are sufficiently open to permit light to enter freely when at night the pupil is dilated: three distinct images of distant lamps are then plainly seen; this is incompatible with the accommodation hypothesis. *Vide Phil. Mag.* S. 4. vol. xxxii. p. 428, § 16.

† In that paper I showed that if an opaque object be looked at attentively in a good light so as to be perfectly well seen,  $D$  being the distance of the object from the cornea, and  $\delta$  the diameter of the pupil, or of any other diaphragm held quite close to the cornea which permits a conical pencil of rays to pass on into the eye, then is  $\frac{\delta}{D}$  a constant quantity; and the rule holds good up to a metre or two, beyond which absolute definition ceases. The angle answering to  $\frac{\delta}{D}$  is about  $20'$  in a fair light; but its value decreases as the degree of illumination increases, constant, however, for the same light.

In the paper above alluded to I have likewise shown that, as far as can be at present surmised, the impulses are received in each case on a portion of the retina of which the diameter is exactly equal to the depth of the sentient layer of that part.

The appearance of printed matter seen through a narrow slit (0.25 mil-lin. in breadth) is very curious, and merits further examination: close along the edges of the slit, above and below, the lines of letters are distinctly seen, the central portion a heap of short lines in singular disorder.

Lastly, I may add that I have been able many times to make out indistinct objects by reducing the size of the diaphragm supplied by the opticum; and I think Sir John Herschel makes a remark which comes to much the same thing, if I remember right, in his '*Cape Observations.*'



opportunity of examining the eyes in different classes of animals, and have come to the conclusion that the retina does not correspond in form to a caustic surface of revolution, but that its form is in a greater or less degree determined by the amount of mobility required and by the construction of the head, or, in other words, simple adaptation to what is suitable to the habits and instincts of the animal. I have likewise in the most careful manner separated the lens under water from its sclerotic attachments, suspended it in the water, shut off extrinsic light, and searched for the focus of parallel rays with different sizes and different arrangements of diaphragm, but have totally failed to discover it: nothing has been noticed but an ill-defined diffused brightness, rather more brilliant in the middle; nothing to force upon the mind the conviction that it acts as a common corrected lens would do; but as the experiments, however carefully made, carry with them a suspicion of inexactness, I do not venture to affirm this too positively.

The eye, like other organs of sense, has its active and passive states; it is often no more sensible of the presence of external objects than the ear to accustomed sounds, or the skin to the presence of the clothes; but the moment attention is excited it is roused into full activity, and perception becomes possible; the iris, controlling the size of the pencils, the lens concentrating the impulses, then determine the exact conditions favourable to vision, and, it may be said, act such a part as an ear-trumpet to a dull ear, by supplying to the retina the exact impressions that are required to be conveyed to the sensorium.

*A priori*, it ought not to be expected that the retina would present a simple surface on which to receive a picture; the waves which compose white light are of different lengths and arrive with different velocities, so that, leaving the source of emanation at the same instant, they must arrive at the retina in very different phases of their length, requiring a certain depth of retinal matter for their full development, a depth not less than  $1\frac{1}{2}$  the length of a medium wave, or about 0.00084 millim. This is just one-fiftieth part of the depth of the rods and bulbs, which are with good reason believed to be the sentient receptacle of the retina. There is therefore ample depth in the body of the retina for the reception of rays from whatever group of external objects the mind be prepared to select. It would seem probable, therefore, that the impressions are received within the substance of the retina, and that the iris and lens between them regulate the intensity of the impulses: time moreover is certainly required by the retina for the impulses to become adjusted to meet the requirements of vision, part of this time being known as "personal equation," the whole of it being no more

than about one-sixth of a second. Much about the functions of the eye is without doubt known; but I strongly insist that further inquiry should take place before it be assumed that "the known properties of light afford a complete elucidation of the whole mechanism of vision and the use of every part of the visual apparatus."

III. *Note on certain Slates, Felsites, and Elvanites occurring near Knockmahon in the County of Waterford.* By J. ARTHUR PHILLIPS\*.

THESE rocks are exposed in the sea-cliff east of the village of Knockmahon, and are much contorted, their general strike being from N.E. to S.W. The dykes or bands of felsite and elvanite often appear to be interstratified with the Lower Silurian fossiliferous slates of the district, and to follow the line of their bedding without producing any marked change in the character of the rocks in immediate contact with them. They can in many instances be traced for considerable distances, but appear to be frequently lenticular in shape, thinning out horizontally at either end, whilst the workings in the adjacent mines have afforded examples of a similar dying out in depth.

At the extreme south-west corner of the townland of Tankardstown is a well-defined north and south fault, immediately to the west of which the rocks are traversed from N.W. to S.E. by the productive copper-bearing lodes of Knockmahon.

Four specimens of the more characteristic rocks from the above locality were selected for microscopical and chemical examination, and afforded the following results.

*Elvanite*, spec. grav. = 2.66.—A section was prepared from a band of compact elvanite of a bluish-grey colour, which, when freshly broken, shows imbedded crystals of quartz and felspar in an amorphous matrix. This band, which is of considerable but varying width, is in the immediate vicinity of the Tankardstown lode, and has been intersected by the workings of the mine at a considerable distance from the cliff.

Examined under a  $\frac{1}{2}$ -inch objective, this rock is seen to be composed of an amorphous greyish matrix in which are porphyritically imbedded crystals of quartz and felspar, the latter being chiefly oligoclase. In addition to these, a few small crystals of some hornblende mineral are sparingly disseminated throughout the mass. It was further observed that the larger quartz crystals are sometimes penetrated by crystals both of felspar and hornblende; and when examined under a high power, the quartz is seen to contain fluid-cavities.

\* Communicated by the Author.

The composition of this rock, as obtained by analysis, is as follows:—

	I.	II.
Water* . . . . .	1·86	1·80
Silica . . . . .	72·33	72·33
Alumina . . . . .	8·97	9·07
Peroxide of iron . .	6·40	6·27
Protoxide of iron . .	1·06	1·06
Lime . . . . .	1·98	1·86
Magnesia . . . . .	trace	trace
Potassa . . . . .	1·46	1·46
Soda . . . . .	5·83	5·83
	<hr/> 99·89	<hr/> 99·68

*Felsite*, spec. grav. = 2·64.—A section was cut from a specimen of flesh-coloured felsite obtained from a very broad band of this rock seen in the cliff near the village of Annestown, and immediately west of a copper vein on which some explorations are being made.

Under the microscope this was found to consist of a colourless and generally amorphous matrix enclosing a few dodecahedral crystals of quartz and some small crystals of felspar.

Other portions of the matrix appear to be indistinctly crystalline, and to enclose a few laminæ of a greenish mineral, probably chlorite. A chemical analysis of this rock afforded results as under:—

Water† . . . . .	1·38
Silica . . . . .	80·50
Alumina . . . . .	8·33
Peroxide of iron . .	3·44
Protoxide of iron . .	·96
Oxide of manganese .	trace
Lime . . . . .	1·21
Magnesia . . . . .	trace
Potassa . . . . .	1·89
Soda . . . . .	2·12
	<hr/> 99·83

*Columnar Slate*, spec. grav. = 2·66.—A specimen from a band of highly metamorphosed slate, which makes its appearance a little to the east of the Stage lode, was next selected for examination.

This rock, which is of a bluish-grey colour, is compact and cherty in its structure, and is divided by a double cleavage into a series of columnar masses, the planes of division of which are

\* Of which ·35 was lost in the water-bath.

† Of which ·37 was lost in the water-bath.

frequently much coloured by peroxide of iron. A section of this slate, made parallel to one of its lines of cleavage, when examined under the microscope, was found to consist of an amorphous matrix through which is somewhat thickly disseminated a flocculence of a dirty greenish colour, perhaps due to the presence of minute quantities of chlorite. A few well-defined quartz-crystals were also apparent. Its chemical composition was found to be as follows :—

	I.	II.
Water* . . . . .	·80	·80
Silica . . . . .	74·36	74·19
Alumina . . . . .	7·66	7·90
Peroxide of iron . . .	7·00	7·20
Protoxide of iron . . .	·90	·90
Lime . . . . .	1·12	1·04
Magnesia . . . . .	·48	·13
Potassa . . . . .	2·10	2·00
Soda . . . . .	5·23	4·87
	99·65	99·33

*Metamorphosed Slate?*, spec. grav. = 2·65.—A section prepared from a band, apparently of highly metamorphosed slate, lying to the east of the foregoing, in which the cleavage-planes had to a great extent become obliterated, was found under the microscope to be chiefly composed of felspathic-looking crystals crossing each other in all directions, with here and there some minute scales of chlorite. These crystals, which readily depolarize polarized light, are nearly transparent; but the small amount of potassa, soda, and lime present in the rock, as shown by analysis, renders it improbable that so large a proportion of it can consist of any variety of felspar.

Its composition, as found by analysis, is as follows :—

Water ‡ . . . . .	5·50
Silica . . . . .	52·66
Alumina . . . . .	18·33
Peroxide of iron . . .	10·43
Protoxide of iron . . .	9·00
Lime . . . . .	1·76
Magnesia . . . . .	trace
Potassa . . . . .	·73
Soda . . . . .	2·12
	100·53

\* Of which ·34 was lost in the water-bath.

† In the second analysis a small loss was experienced from spirting when evaporating.

‡ Of which ·50 was lost in the water-bath.

*Mean Results.*—Taking the means of the two analyses made in duplicate, and tabulating the results for facility of comparison, we obtain the following :—

	Elvanite.	Felsite.	Columnar slate.	Metamorphosed slate (?)
Water.....	1·83	1·38	·80	5·50
Silica .....	72·33	80·50	74·28	52·66
Alumina .....	9·02	8·33	7·78	18·33
Peroxide of iron .....	6·34	3·44	7·10	10·43
Protoxide of iron .....	1·06	·96	·90	9·00
Oxide of manganese ...	.....	trace		
Lime .....	1·92	1·21	1·08	1·76
Magnesia .....	trace	trace	·46	trace
Potassa .....	1·46	1·89	2·05	·73
Soda .....	5·83	2·12	5·05	2·12
	99·79	99·83	99·50	100·53
Loss in water-bath ...	·35	·37	·34	·50

It will be observed that the two specimens of rock most unlike in general appearance, and which, when microscopically examined, were found to differ most essentially in their structural arrangement, resemble each other very closely in point of chemical composition. In fact the ultimate chemical constitutions of the elvanite and of the columnar clay-slate are so nearly alike, that a slight change in the latter, resulting in the rearrangement and crystallization of certain of its constituents, might evidently lead to the production of the former.

The compositions of the two other rocks differ essentially, not only between themselves, but also very materially from those of the specimens to which we have referred above.

*Analyses.*—In making these analyses, the following general routine was adopted.

The amount of moisture present was ascertained by heating a weighed quantity of the finely pulverized rock in a water-bath until it ceased to lose weight, when it was strongly ignited and the further loss noted.

About thirty grains of the powdered substance was intimately mixed with three times its weight of carbonate of soda in a platinum crucible. This was enclosed in a clean earthen one, and fused in an ordinary air-furnace supplied with hard coke.

The fused mixture, after being allowed to cool, was treated with hydrochloric acid and evaporated to dryness. The dried residuum was then moistened with hydrochloric acid, water added, and solution of the chlorides aided by ebullition. The *silica*, now separated by filtration, was washed, dried, and ignited. From the filtrate, after peroxidation of the iron, *oxide of iron and*



*alumina* were precipitated by ammonia; and after being washed and ignited, their united weight was noted.

The separation of *peroxide of iron* from *alumina* was effected by dissolving the mixture in dilute nitro-hydrochloric acid, and precipitated by an excess of pure caustic potassa. The peroxide of iron thus obtained, after being redissolved and precipitated by ammonia, was washed and weighed. The *alumina* was obtained by acidifying the alkaline solution and precipitating by ammonia.

A small quantity of *silica* was frequently obtained on attacking the mixture of peroxide of iron and *alumina*, and its weight was added to that previously found.

In order to determine the respective amounts of *protoxide* and *peroxide* of iron, a separate fusion was made on a weighed quantity of the pulverized rock and sesquicarbonate of soda, care being taken to cover the surface of the mixture with a layer of the pure sesquicarbonate.

The contents of the crucible were thus both fused and allowed to cool in an atmosphere of carbonic acid, besides being preserved from oxidation by a covering of carbonate of soda.

The crucible and its contents were now placed in a covered beaker containing dilute hydrochloric acid, and heated on the sand-bath; and as soon as carbonic acid ceased to be evolved, the solution was allowed to cool, without removing the cover, and the quantity of *protoxide of iron* present estimated by a freshly standardized solution of permanganate of potassa.

The protoxide thus found was subsequently converted, by calculation, into sesquioxide, and deducted from the amount obtained by direct estimation. The *lime* was precipitated in the filtrate separated from the mixture of oxide of iron and *alumina* by the addition of oxalate of ammonia; and to the filtrate from the lime-salt phosphate of soda was added for the precipitation of *magnesia*.

The percentage of the alkalies was determined by fusion with caustic baryta, dissolving in hydrochloric acid, and evaporating to dryness. Baryta-water and carbonate of ammonia were subsequently added, and the weight of mixed chlorides obtained. To the solution of these, bichloride of platinum was added, and the weight of *potassa* determined in the usual way. The amount of *soda* was ascertained by deducting from the weight of mixed chlorides the chloride of potassium corresponding to the *potassa* found, and calculating the quantity of *soda* corresponding to the remainder.



IV. *Observations of the Corona during the Total Eclipse, August 7, 1869.* By G. K. WINTER, *Telegraph Engineer, Madras Railway Company, Arcunum.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

IN the October Number of the *Philosophical Magazine* there is an article by Professor Pickering entitled "Observations of the Corona during the Total Eclipse, August 7th, 1869." As the results obtained by Professor Pickering by means of the double-image polariscope are directly at variance with the experience of myself and others during the eclipse of August the 18th, 1868, I hope a few remarks upon the subject may be deemed worthy of your acceptance.

In August 1868 I accompanied Mr. Pogson (the Government Astronomer at Madras) to Masulipatam, to assist him in observing the now celebrated eclipse of that year. My special duty was to test the light of the corona and red flames for polarization. For this purpose I was provided with a Savart's polariscope, and a double-image prism with a plate of quartz cut perpendicularly to the axis. Each of these polariscopes was adapted to an excellent telescope by Cooke and Sons,  $2\frac{1}{2}$ " aperture and 28" focal length.

On examining the light from the corona by means of the Savart's polariscope, I found it very strongly polarized everywhere in planes radial to the sun's centre. The bands were extremely vivid near the moon's limb, and faded off with the corona. There was very little light, polarized or unpolarized, beyond the limits of the corona; so that the explanation that the polarized light seen by some observers was derived from the atmosphere illuminated by the unobscured portions of the earth is, I think, untenable. Several portions of the corona were successively tried; and in all the white central bands were brightest where they were radial to the sun, and the black-centred bands were equally marked when they were tangential to the limb.

My observations were completely confirmed by those made by Captain Branfill, of Major Tennant's expedition.

Instead of being negative, I consider the results obtained by the polariscope to have been conclusive as to the cause of the corona of the eclipse of 1868. In order to give a reflecting angle calculated to produce the amount of polarization observed, the reflecting atmosphere must have surrounded the sun.

Yours very truly,

G. K. WINTER.

V. *A Mathematical Theory of Tides.* By the Rev. Professor  
CHALLIS, M.A., F.R.S., F.R.A.S.\*

THE problem of the tides of oceans, which was pronounced by Laplace to be “le plus épineux de toute la Mécanique Céleste,” is also one which, when put in its simplest form, especially requires to be treated in strict accordance with the necessary principles of hydrodynamics. In the following attempt to give it such a solution, on the supposition that the whole of the solid part of the earth is covered by water of a certain limited depth, I commence with employing the general equations applicable to the motion of an incompressible fluid. The mode of applying these equations in this problem, and the subsequent course of the reasoning, are, as far as I am aware, such as have not been adopted before.

The resolved parts of the velocity  $V$  at the point  $xyz$  at the time  $t$  being  $u, v, w$  in the directions of any axes of rectangular coordinates, we have for an incompressible fluid the known general equation

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0. \quad . \quad . \quad . \quad . \quad (\alpha)$$

This equation expresses that the quantity of fluid in a given small rectangular space remains the same, in whatever way the fluid passes through it. I assume as an axiom the existence of *surfaces of displacement* (that is, of surfaces which cut at right angles the directions of the motion), which may also be discontinuous in any manner so long as they are not *broken* or their tangent planes do not at any point make a finite angle with each other. Consequently the equation  $u dx + v dy + w dz = 0$ , which is capable of satisfying these conditions, is the general differential equation of the surfaces of displacement, provided the left-hand side be integrable of itself. If it is not, it must, according to the above axiom, admit of exact integration by means of a factor.

In that case, the factor being  $\frac{1}{\lambda}$ , the general differential equation of the surfaces of displacement is

$$\frac{u}{\lambda} dx + \frac{v}{\lambda} dy + \frac{w}{\lambda} dz = 0.$$

In *both* these cases, as I have elsewhere shown,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = \frac{dV}{ds} + V \left( \frac{1}{r'} + \frac{1}{r''} \right),$$

$dV$  being the increment of the velocity at a given instant corre-

\* Communicated by the Author

sponding to the increment  $ds$  of space in the direction of the motion, and  $r'$ ,  $r''$  the principal radii of curvature of the surface of displacement at the point  $xyz$ . (See the proof of this equality in the *Philosophical Magazine* for March 1850, p. 173, and in the 'Principles of Mathematics,' p. 183.)

It is known that there are instances of the motion of a fluid mass in which either the whole of it moves as if it were solid, or it may be conceived to consist of infinitesimal portions each of which has the motion of a solid. In such cases  $r'$  and  $r''$  will both be infinitely great, because the lines of motion in each element will be parallels, and the surfaces of displacement will consequently be planes. We shall thus have  $\frac{dV}{ds} = 0$ , or  $V$  a function of  $t$  along a given line of motion.

But whenever  $r'$  and  $r''$  are not both infinite, it is evident that the parts of a given element are changing their relative positions, and that consequently the motion is incompatible with the solid state, and is distinctively that of a fluid. In all these instances the surfaces of displacement are *curved* surfaces of continuous curvature, or are made up of parts that are such, and their general differential equation is  $udx + vdy + wdz = 0$ . Hence for motion which is proper to a fluid, the left-hand side of that equation is integrable of itself. Since also, from the foregoing argument, that differential quantity is always either an exact differential or integrable by a factor, it follows that the analytical circumstance of its being integrable by a factor indicates that the motion of each infinitesimal portion is like that of a solid. It may also be the case that the total motion is partly of one kind and partly of the other; but in such instances it is possible to separate one from the other and treat them independently. By these considerations I come to the general conclusion that  $udx + vdy + wdz$  is an exact differential so far as it applies to motion which is peculiar to a fluid. This theorem, although I had not recognized it before, is not inconsistent with the results of my previous hydrodynamical researches.

Putting  $(d\phi)$  for  $udx + vdy + wdz$ , the equation  $(\alpha)$  becomes

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0; \quad . \quad . \quad . \quad . \quad . \quad (\beta)$$

at the same time the integration of the equation

$$\frac{dV}{ds} + V\left(\frac{1}{r'} + \frac{1}{r''}\right) = 0$$

gives  $V = \frac{\phi(t)}{r' r''}$ . This result is to be regarded as an integral of the equation  $(\beta)$ , because, from what is shown above, the two

equations are identical, inasmuch as *both* apply to motion which is peculiar to a fluid. As this expression for  $V$  has been obtained prior to the consideration of any particular case of motion, it must be applicable in all cases, and at every point of the fluid mass at all times. Consequently it partakes of the character of a *general integral*, but gives the value of the velocity, in any particular case of the disturbance of the fluid, only through an infinitesimal portion of fluid, for which the radii of curvature of the surfaces of displacement may be supposed to have, at a given instant, the same positions. If such a portion be bounded laterally by planes coincident in direction with lines of motion, any transverse section of the small included element will be proportional to the product  $r'r''$ . Hence, if the velocity and transverse section be  $V_1$  and  $m_1^2$  at one end of the element, and  $V_2$  and  $m_2^2$  at the other, we shall have  $V_1 m_1^2 = V_2 m_2^2$ . Then passing to the next element, and supposing  $r'$  and  $r''$  to change in magnitude and position either continuously or discontinuously, but not *per saltum*, we have similarly  $V_2 m_2^2 = V_3 m_3^2$ ; and so on. This reasoning proves that if the whole of the fluid be divided into filaments of indefinitely small transverse section, having their lateral boundaries everywhere coincident in direction with lines of motion, and the value of  $V_1 m_1^2$  at a certain transverse section of a filament be given, the value  $V m^2$  at any other transverse section is also given. This inference is evidently consistent with the principle of the constancy of mass, and is independent of the form and position of the filament, which have to be deduced from the given conditions of the particular case of motion.

The general integral of the equation ( $\beta$ ) may, as Poisson has shown, be expressed under a form which involves arbitrary functions of impossible quantities. This analytical result might give the means of arriving at that form of the general integral which is obtained above by a different process; but actually it renders the integral unfit for application to the particular circumstances of a case of motion. In fact, as the equation ( $\beta$ ) takes no account of the impressed forces, its general integral cannot possibly admit of specific applications to given cases of disturbance of the fluid, and can only have the general signification which I have just indicated. This remark will be further illustrated by the proposed method of solving the problem of the tides.

In the subsequent analytical reasoning a differential is put in brackets to show that the differentiation has relation to coordinates only; a differential coefficient in brackets is a complete differential coefficient, with respect to time, of a function of coordinates and the time; all other differential coefficients are partial. This being understood, I proceed next to adduce some



preliminary considerations respecting the general hydrodynamical equation

$$(dp) = \left( X - \left( \frac{du}{dt} \right) \right) dx + \left( Y - \left( \frac{dv}{dt} \right) \right) dy + \left( Z - \left( \frac{dw}{dt} \right) \right) dz \\ = Xdx + Ydy + Zdz - \left( \frac{du}{dt} \right) dx - \left( \frac{dv}{dt} \right) dy - \left( \frac{dw}{dt} \right) dz, \quad . \quad . \quad (\gamma)$$

in which the impressed accelerative forces in the directions of the axes of coordinates are  $X$ ,  $Y$ ,  $Z$ . It will be assumed that  $Xdx + Ydy + Zdz$  is an exact differential ( $dF$ ), so that

$$\left( \frac{du}{dt} \right) dx + \left( \frac{dv}{dt} \right) dy + \left( \frac{dw}{dt} \right) dz = (dF) - (dp). \quad . \quad (\delta)$$

This equation proves that the left-hand side of this equation is an exact differential in *all* cases, if only the impressed forces be such as to make  $Xdx + Ydy + Zdz$  an exact differential. The condition of integrability is therefore satisfied both when each element moves as if it were solid, and when the motion in each element is characteristically that of a fluid. We have seen that in the latter case  $udx + vdy + wdz$  is always an exact differential, and that in the former the same differential quantity is integrable by a factor. It thus appears that the left-hand side of the equation ( $\delta$ ) is made integrable by putting  $(d\phi)$ , or else  $\lambda(d\psi)$ , for  $udx + vdy + wdz$ .

In the cases in which  $(d\phi)$  may be substituted for  $udx + vdy + wdz$ , the integral of the equation ( $\delta$ ) is, as is well known,

$$p = F - \frac{d\phi}{dt} + \frac{V^2}{2} + \psi(t),$$

an arbitrary function of the time, which may also contain constants, being added, because, as the differential is complete, the integration may be taken from a fixed point of space in the fluid to any other point in the same.

It may here be remarked, since

$$\left( \frac{du}{dt} \right) dx + \left( \frac{dv}{dt} \right) dy + \left( \frac{dw}{dt} \right) dz = \left( \frac{dV}{dt} \right) ds,$$

and since the integral  $\int \left( \frac{dV}{dt} \right) ds$  may be taken from any one point to any other of the fluid, that the value of  $F$  undergoes no change in passing from point to point of a given surface of displacement at a given time, because in that case the variation  $ds$  in the direction of the motion vanishes. Hence we may infer that if  $udx + vdy + wdz$  be an exact differential, the function  $F$  has the same value at all points of the same surface of displacement.

We may now proceed to apply these preliminary considerations

in the problem of the tides. In order to simplify the reasoning, it will be supposed (1) that the solid part of the earth is spherical; (2) that this part is covered by water, the depth of which is small compared with the earth's radius; (3) that the moon revolves about the earth in the plane of the equator at her mean distance with the mean angular velocity. We may, if we please, abstract from the earth's rotation by conceiving an equal angular velocity to be impressed on the moon in the opposite direction.

Conceiving, therefore, the earth to have no motion, and taking its centre for the origin of rectangular coordinates, let the axis of  $z$  coincide with its polar axis, and the axis of  $x$  pass through the meridian of Greenwich. Then if  $\lambda$  be the north latitude, and  $\theta$  the west longitude, of any particle of the fluid distant by  $r$  from the origin, we shall have

$$x = r \cos \lambda \cos \theta, \quad y = r \cos \lambda \sin \theta, \quad z = r \sin \lambda.$$

If  $G$  be the usual measure of gravity at the upper surface of the fluid at a certain distance  $a$  from the centre of the spherical mass, the force at any point in the fluid at the distance  $r$  will, on account of the supposed small depth, be very nearly  $G \left( 1 + k \frac{a-r}{a} \right)$ ,

whether the mass be homogeneous or not. The factor  $k$  depends on the ratio of the density of the water, or generally of the superficial strata, to the earth's mean density, and may readily be calculated if that ratio be given. For instance, I have found that if the ratio be one-sixth,  $k = \frac{2}{3}$ ; if one-third,  $k = 1$ .

Again, let  $m$  be the moon's attraction at the unit of distance, referred to the same unit of measure as  $G$ ,  $R$  the mean distance, and  $\mu t$  her angular distance westward from the meridian of Greenwich at the time  $t$  reckoned from the meridian transit, the apparent angular velocity  $\mu$  being equal to the excess of the earth's rate of rotation above the moon's mean motion. Then, the centrifugal force due to the earth's rotation being  $\omega^2$  at the unit of distance from the axis, the usual investigation to the first power of the ratio of  $r$  to  $R$  gives

$$X = -G \left( 1 + k \frac{a-r}{a} \right) \frac{x}{r} + \omega^2 x + \frac{m}{R^3} \left( x(3 \cos^2 \mu t - 1) + \frac{3y}{2} \sin 2\mu t \right),$$

$$Y = -G \left( 1 + k \frac{a-r}{a} \right) \frac{y}{r} + \omega^2 y + \frac{m}{R^3} \left( y(3 \sin^2 \mu t - 1) + \frac{3x}{2} \sin 2\mu t \right),$$

$$Z = -G \left( 1 + k \frac{a-r}{a} \right) \frac{z}{r} - \frac{mz}{R^3}.$$

Hence  $Xdx + Ydy + Zdz$  is in this instance an exact differential, and  $(dF)$  may be substituted for it. Hence also we may put



( $dQ$ ) for the left-hand side of the equation ( $\delta$ ); and because tidal motion is evidently such as is proper to a fluid, we have besides ( $d\phi$ )  $= udx + vdy + wdz$ .

It is readily seen that integration gives

$$F = -G \left( r - k \frac{(a-r)^2}{2a} \right) + \frac{\omega^2 r^2}{2} \cos^2 \lambda \\ + \frac{m}{2R^3} (x^2(3 \cos^2 \mu t - 1) + y^2(3 \sin^2 \mu t - 1) + 3xy \sin 2\mu t - z^2).$$

Hence, since by integrating ( $\delta$ ) we have  $p = F - Q + \psi(t)$ , after substituting the values of  $x, y, z$ , the result will be found to be

$$\left. \begin{aligned} p = -G \left( r - k \frac{(a-r)^2}{2a} \right) + \frac{\omega^2 r^2}{2} \cos^2 \lambda \\ + \frac{mr^2}{2R^3} (3 \cos^2 \lambda \cos^2 (\theta - \mu t) - 1) - Q + \psi(t). \end{aligned} \right\} \quad (\epsilon)$$

As the proposed method of solution is necessarily approximative, small quantities of the second and higher orders with respect to the disturbing force, as also the terms containing  $k$  and  $\omega^2$ , will at first be omitted, and it will afterwards be shown how the omitted terms might be taken into account. Hence, putting  $\frac{d\phi}{dt}$  for  $Q$ ,

$$p = -Gr + \frac{mr^2}{2R^3} (3 \cos^2 \lambda \cos^2 (\theta - \mu t) - 1) - \frac{d\phi}{dt} + \psi(t). \quad (\eta)$$

It is next required to obtain from this equation the complete differential of  $p$  with respect to  $t$ —that is, the variation of  $p$  for a *given* particle in a given indefinitely small interval of time. In the differentiation for this purpose,  $r, \lambda$ , and  $\theta$  will be considered constant in the term containing  $m$ , because their variations would introduce terms of the second order relative to the disturbing force. Consequently

$$\left( \frac{dp}{dt} \right) = -G \frac{dr}{dt} + \frac{3mr^2\mu}{2R^3} \cos^2 \lambda \sin 2(\theta - \mu t) - \frac{d^2\phi}{dt^2} + \psi'(t).$$

Now let the resolved parts of the velocity in the direction of the radius vector produced, and in the two directions at right angles to this, westward and northward, be respectively  $u', v',$  and  $w'$ . Then

$$u' = \frac{dr}{dt} = \frac{d\phi}{dr}, \quad v' = r \cos \lambda \frac{d\theta}{dt} = \frac{d\phi}{r \cos \lambda d\theta}, \quad w' = \frac{rd\lambda}{dt} = \frac{d\phi}{rd\lambda}.$$

Hence by partial differentiation of the above equation

$$\frac{d \cdot \left( \frac{dp}{dr} \right)}{dr} = -G \frac{du'}{dr} + \frac{3mr\mu}{R^3} \cos^2 \lambda \sin 2(\theta - \mu t) - \frac{d^2 u'}{dt^2},$$

$$\frac{d \cdot \left( \frac{dp}{d\theta} \right)}{d\theta} = -G \frac{du'}{d\theta} + \frac{3mr^2\mu}{R^3} \cos^2 \lambda \cos 2(\theta - \mu t) - r \cos \lambda \frac{d^2 v'}{dt^2},$$

$$\frac{d \cdot \left( \frac{dp}{d\lambda} \right)}{d\lambda} = -G \frac{du'}{d\lambda} - \frac{3mr^2\mu}{R^3} \sin 2\lambda \sin 2(\theta - \mu t) - r \frac{d^2 w'}{dt^2}.$$

We have now to introduce the condition that a particle at the upper surface of the fluid remains at the surface in successive instants, so that for every such particle  $\left( \frac{dp}{dt} \right) = 0$ , because the pressure is that of the atmosphere, which is supposed to be constant. Hence the left-hand sides of the three preceding equations vanish for a superficial particle. Thus the first of these equations becomes

$$0 = -G \frac{du'}{dr} + \frac{3mr\mu}{R^3} \cos^2 \lambda \sin 2(\theta - \mu t) - \frac{d^2 u'}{dt^2},$$

and, being applicable to every point of the surface at all times, gives the means of determining what function  $u'$  is of the variables  $r$ ,  $\theta$ ,  $\lambda$ , and  $t$ . It must, however, be noticed that it is not the general integral of this partial differential equation that the solution of the problem demands, but a *particular* integral of definite form, also that there cannot be more than one such integral suitable to the given circumstances. It is evident that the integral required for the case of an unbounded ocean will be obtained by supposing that

$$u' = \Lambda \phi(r) \cos^2 \lambda \sin 2(\theta - \mu t),$$

without adding an arbitrary function of  $\theta$  and  $\lambda$ , for determining which there are no conditions. For by substitution we have

$$0 = -G \Lambda \phi'(r) + \frac{3m\mu r}{R^3} + 4\Lambda \mu^2 \phi(r),$$

an equation proper for determining the form of the function  $\phi(r)$ . In fact, putting for brevity  $q$  for  $\frac{4\mu^2 a}{G}$ , it will be found

by integrating that the equation is satisfied if

$$A = \frac{3m\mu a}{GR^3}, \quad \text{and} \quad \phi(r) = -\frac{1}{q^2}(a + qr) + Ce^{\frac{qr}{a}},$$

C being the arbitrary constant introduced by the integration. Consequently

$$u' = -\frac{3m\mu a}{GR^3} \left( \frac{a + qr}{q^2} - Ce^{\frac{qr}{a}} \right) \cos^2 \lambda \sin 2(\theta - \mu t).$$

By means of this value of  $u'$  the values of  $v'$  and  $w'$  may be obtained from the equations

$$0 = -G \frac{du'}{d\theta} + \frac{3mr^2\mu}{R^3} \cos^2 \lambda \cos 2(\theta - \mu t) - r \cos \lambda \frac{d^2 v'}{dt^2},$$

$$0 = -G \frac{du'}{d\lambda} - \frac{3mr^2\mu}{R^3} \sin 2\lambda \sin 2(\theta - \mu t) - r \frac{d^2 w'}{dt^2}.$$

It will hence be found that

$$v' = -\frac{3m\mu a}{GR^3} \left( \frac{r}{q} + \frac{2a}{q^2} + \frac{2a^2}{q^3 r} - \frac{2aC}{qr} e^{\frac{qr}{a}} \right) \cos \lambda \cos 2(\theta - \mu t),$$

$$w' = \frac{3m\mu a}{2GR^3} \left( \frac{r}{q} + \frac{2a}{q} + \frac{2a^2}{q^3 r} - \frac{2aC}{qr} e^{\frac{qr}{a}} \right) \sin 2\lambda \sin 2(\theta - \mu t).$$

These values of  $u'$ ,  $v'$ , and  $w'$  satisfy the condition of making  $u'dr + v'r \cos \lambda d\theta + w'rd\lambda$  an exact differential ( $d\phi'$ ); whence by integration

$$\phi' = -\frac{3m\mu a}{2GR^3} \left( \frac{r^2}{q} + \frac{2ar}{q^2} + \frac{2a^2}{q^3} - \frac{2aC}{q} e^{\frac{qr}{a}} \right) \cos^2 \lambda \sin 2(\theta - \mu t).$$

It is now to be observed that although the form of the function  $\phi(r)$ , and the above expressions for  $u'$ ,  $v'$ ,  $w'$ , have been found by satisfying the circumstances of the motion and pressure at the limits of the fluid mass, the same forms are applicable at *any* point of the interior. The reason for this assertion is, that as the motion at each point is determined by the given arbitrary conditions, expressions applying to the motion generally can only be obtained by satisfying these conditions. Or it may be said that it is only as being generally applicable that any expressions are capable of satisfying the given conditions. The principle here stated has been admitted by Poisson in his solu-

tion of the problem of the resistance of the air to the motion of a ball-pendulum.

This point being allowed, the arbitrary constant  $C$  will enable us to introduce the condition that the inferior limit of the water is a spherical surface of constant radius. For, putting  $b$  for this radius, we shall thus have  $u'=0$  where  $r=b$ , and consequently

$$C = \frac{a+qb}{q^2} e^{-\frac{qb}{a}}.$$

After the substitution of this value of  $C$  in the expressions for  $\phi'$ ,  $u'$ ,  $v'$ ,  $w'$ , the functions of  $r$  in brackets may be expanded in terms proceeding according to the powers of the small quantity  $q$ , which is of the order of the ratio of the centrifugal force at the earth's equator to the force of gravity. When this is done and terms wholly insignificant are omitted, the results are

$$\phi' = -\frac{3m}{8R^3\mu} \left( b^2 - \frac{4\mu^2}{3G} (r-b)^2(r+2b) \right) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$u' = \frac{3m\mu}{2GR^3} (r^2 - b^2) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$v' = -\frac{3mb^2}{4R^3\mu r} \left( 1 - \frac{4\mu^2}{3Gb^2} (r-b)^2(r+2b) \right) \cos \lambda \cos 2(\theta - \mu t),$$

$$w' = \frac{3mb^2}{8R^3\mu r} \left( 1 - \frac{4\mu^2}{3Gb^2} (r-b)^2(r+2b) \right) \sin 2\lambda \sin 2(\theta - \mu t).$$

These approximate values of  $u'$ ,  $v'$ ,  $w'$  satisfy the equality

$$(d\phi') = udr + v'r' \cos \lambda d\theta + w'r d\lambda.$$

It will be seen that  $u'=0$  at the points for which  $\lambda = \pm \frac{\pi}{2}$ , and that consequently the radius vector of the surface is at these points invariable. We may therefore suppose the distance  $a$  to be this value of the radius vector, so that  $G$  is the force of gravity at the two poles.

Resuming the equation ( $\eta$ ), and substituting in it for  $\frac{d\phi}{dt}$  from the above value of  $\phi'$ , the resulting equation for calculating  $p$  is

$$p = -Gr + \frac{mr^2}{2R^3} (3 \cos^2 \lambda \cos^2 (\theta - \mu t) - 1) \\ - \frac{3m}{4R^3} \left( b^2 - \frac{4\mu^2}{3G} (r-b)^2(r+2b) \right) \cos^2 \lambda \cos 2(\theta - \mu t) + \psi(t).$$

Let  $\varpi$  be the atmospheric pressure. Then, since  $\lambda = \pm \frac{\pi}{2}$  at the points of the surface for which  $r=a$ , we have

$$\varpi = -Ga - \frac{ma^2}{2R^3} + \psi(t),$$

which equation determines the arbitrary quantity  $\psi(t)$ . It will hence be found that

$$\begin{aligned} p - \varpi = & G(a-r) + \frac{m}{2R^3} \left( a^2 - r^2 + \frac{3r^2}{2} \cos^2 \lambda \right) \\ & + \frac{3m}{4R^3} \left( r^2 - b^2 + \frac{4\mu^2}{3G} (r-b)^2 (r+2b) \right) \cos^2 \lambda \cos 2(\theta - \mu t). \end{aligned}$$

By supposing that  $p = \varpi$ , the equation of the exterior surface of the ocean at any time  $t$  is obtained.

From this first approximation the main features of ocean-tides are readily deducible. But before drawing any inferences, a remark of considerable importance must be made, in order to meet an objection that might be raised against the previous reasoning. It appears that the equation ( $\beta$ ), viz.

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0,$$

is *not* satisfied when the foregoing value of  $\phi'$  is substituted in it. I have already intimated that this was to be expected, because, if the value of  $\phi$  which applies to the given circumstances of the motion satisfied the equation ( $\beta$ ), it should be derivable from the general integral of that equation. But this is impossible, inasmuch as the equation takes no account of the impressed forces. The explanation of this apparent difficulty is, that the equation ( $\beta$ ), while it expresses the principle of constancy of mass, has no other general integral than the

one I have already obtained, viz.  $V = \frac{\phi(t)}{r^J r''}$ . In any particular

case of motion this value of  $V$  may be assumed to be the same as that in a given line of motion, but only *through an indefinitely small portion of it*, for which the magnitudes and positions of the radii of curvature  $r^J$  and  $r''$  may be assumed to be constant. By treating these magnitudes and positions as variable parameters, the coincidence of the values of  $V$  may be made to extend through the whole length of the line; and so for any other line. The analytical circumstances are therefore analogous to those of the general integral and particular solution of certain differen-



tial equations. The latter, although it gives the solution of the proposed problem, is not directly obtainable by integrating the given differential equation, but may be deduced by varying the parameters of the general integral. It is thus that in every case of the solution of a hydrodynamical problem by a particular and definite integral, the general equation  $V = \frac{\phi(t)}{r^{\frac{1}{2}} p^{\frac{1}{2}}}$  is satisfied at each point, if only that integral satisfies the condition of making  $u dx + v dy + w dz$  an exact differential. In the instance before us it has been shown that this condition is satisfied; and we may therefore conclude that our integral is in accordance with the principle of constancy of mass expressed by the equation ( $\beta$ ), which is all that exact reasoning demands. But because the integral is not obtainable by direct integration of that equation, I have attached a dash to the symbol  $\phi$  for the sake of distinction. The views here stated have long been advocated by me in my hydrodynamical researches, but they have not, I think, been so clearly exemplified in any previous problem.

The following conclusions are deducible from the foregoing values of  $u'$ ,  $v'$ , and  $w'$ . The sign of the vertical velocity  $u'$  is the same as that of  $\sin 2(\theta - \mu t)$ . Hence, reckoning *westward* from the point to which the moon is vertical, the waters are rising from  $0^\circ$  to  $90^\circ$  of longitude and from  $180^\circ$  to  $270^\circ$ , and falling from  $90^\circ$  to  $180^\circ$  and from  $270^\circ$  to  $360^\circ$ . Hence it is *high water along the meridian under the moon*, and along the opposite meridian, and low water along the meridians  $90^\circ$  distant from these.

The moon being supposed to move in the plane of the earth's equator, the tide on a given meridian varies as the square of the cosine of latitude.

The horizontal velocity in longitude ( $v'$ ) is negative, and therefore *eastward*, at the meridian under the moon, and at the opposite meridian, and *westward* at the meridians  $90^\circ$  from these. At the four meridians midway between these four it vanishes. This velocity varies on a given meridian simply as the cosine of the latitude.

The horizontal velocity in latitude ( $w'$ ) is in directions *from* the equator where the tide is *rising*, and in directions *towards* the equator where the tide is *falling*. It vanishes at the equator and the poles, and has its greatest value in the north and south latitudes of  $45^\circ$ .

The ratio of  $w'$  to  $v'$  is equal to  $-\sin \lambda \tan 2(\theta - \mu t)$ .

Generally, the tides consist of two great waves in opposite hemispheres, stretching from pole to pole, and travelling round the



earth westward at the rate of the moon's apparent diurnal motion.

It remains now to indicate in what manner the small quantities neglected in the first approximation may be taken into account. If we omit, for the present, the terms of the second order with respect to the velocity, the terms neglected in the value of  $p$  were

$$\frac{Gk}{2a} (a-r)^2 + \frac{\omega^2 r^2}{2} \cos^2 \lambda.$$

The complete differentiation of these with respect to time would add in the value of  $\left(\frac{dp}{dt}\right)$  the terms

$$-\frac{Gk}{a} (a-r)u' + \omega^2 r \cos^2 \lambda u' - \frac{\omega^2 r}{2} \sin 2\lambda w',$$

and in that of  $\frac{d \cdot \left(\frac{dp}{dt}\right)}{dr}$  the terms

$$\frac{Gk}{a} \left(u' - (a-r) \frac{du'}{dr}\right) + \omega^2 \cos^2 \lambda u' + \omega^2 r \cos^2 \lambda \frac{du'}{dr},$$

it being supposed that  $\frac{d \cdot rw'}{dr} = 0$ , because with sufficient approximation

$$rw' = \frac{3mb^2}{8R^3\mu} \sin 2\lambda \sin 2(\theta - \mu t).$$

The new terms in the value of  $\frac{d \cdot \left(\frac{dp}{dt}\right)}{d\theta}$  are

$$\left(-\frac{Gk}{a} (a-r) + \omega^2 r \cos^2 \lambda\right) \frac{du'}{d\theta} - \frac{\omega^2 r}{2} \sin 2\lambda \frac{dw'}{d\theta};$$

and those in the value of  $\frac{d \cdot \left(\frac{dp}{dt}\right)}{d\lambda}$  are

$$\begin{aligned} &\left(-\frac{Gk}{a} (a-r) + \omega^2 r \cos^2 \lambda\right) \frac{du'}{d\lambda} - \omega^2 r \sin 2\lambda u' \\ &\quad - \frac{\omega^2 r}{2} \sin 2\lambda \frac{dw'}{d\lambda} - \omega^2 r \cos 2\lambda w'. \end{aligned}$$

All the additional terms are expressible as definite functions of  $r$ ,  $\theta$ ,  $\lambda$ , and  $t$  by means of the first approximations to the values

of  $u'$ ,  $v'$ , and  $w'$ ; and, after being so expressed, more correct values of these velocities can be obtained by new integrations.

It may here be stated that the term  $\frac{Gk}{2a}(a-r)^2$  might have been included in the first integrations. I obtained, in fact, by so doing the following results:

$$\phi(r) = -\frac{(1+k)a + (q+k)r}{(2k+q)(k+q)} + C(a+k(a-r))^{-(1+\frac{q}{k})},$$

$$u' = \frac{3mq}{4R^3\mu} \phi(r) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$v' = -\frac{3m}{4R^3\mu} \left( r - 2(a+k(a-r)) \frac{\phi(r)}{r} \right) \cos \lambda \cos 2(\theta - \mu t),$$

$$w' = -v' \sin \lambda \tan 2(\theta - \mu t).$$

When all the terms in  $u'$ ,  $v'$ ,  $w'$  which contain the first power of  $m$  have thus been obtained with sufficient exactness, we can proceed to the investigation of the terms containing  $m^2$ , by first substituting, in those terms of the second order with respect to velocity which occur in the *complete* values of

$$p, \quad \left( \frac{dp}{dt} \right), \quad \frac{d \cdot \left( \frac{dp}{dt} \right)}{dr}, \quad \frac{d \cdot \left( \frac{dp}{dt} \right)}{d\theta}, \quad \frac{d \cdot \left( \frac{dp}{dt} \right)}{d\lambda},$$

the values of  $u'$ ,  $v'$ ,  $w'$  given by the antecedent approximation, and then effecting new integrations.

I have thus shown how the problem of ocean-tides, under the simple form in which it has been proposed in this communication, may be solved with as much accuracy as we please. As I have neither the leisure nor the inclination to go through the details of the second approximation, I have only indicated the proper process, in the hope that younger mathematicians may be induced, by the great importance of an accurate solution of this problem, to undertake the requisite analytical calculation. The problem has acquired special interest since M. Delaunay suggested, in the course of his researches on the acceleration of the moon's mean motion, that the length of the sidereal day may be affected by tidal action. But the verification of this view evidently demands that the mathematical investigation employed for the purpose should be conducted in strict accordance with the principles of hydrodynamics, and be carried to terms of the second order with respect to the disturbing force. As far as I can judge without the aid of such investigation, it seems to me possible that the increment of pressure at high water on eastern

shores, or against obstacles in ocean-beds to the free propagation of the tidal wave westward, may not be exactly compensated for by the reverse action at low water, and that there is consequently a residual action westward tending to diminish the rate of the earth's rotation. Such an effect would depend on terms of the second order. But whether this cause actually operates, and whether currents produced by the moon's attraction have any effect, we should, I think, have some means of deciding if the problem of tides, as here proposed, were strictly solved to the second approximation.

I beg to conclude this communication by adverting to a question which bears in an important manner on the truth of the foregoing mathematical reasoning. It will be seen that I have inferred from the expression for  $u'$  that it is "high water along the meridian under the moon." In an article contained in the 'Monthly Notices of the Royal Astronomical Society' (vol. xxvi. No. 6), the Astronomer Royal has maintained that, when friction is left out of account, "it is low water under the moon." It is absolutely necessary, for the credit of mathematics, to account for such contradictory results being obtained from the same premises.

In support of his view Mr. Airy cites Newton's *Principia*, Lib. I. Prop. 66, Cor. 19. But Laplace, in his account of Newton's theory of the tides, interprets that passage in a different sense. In the *Mécanique Céleste* (vol. v. p. 146, edit. of 1825) he states that Newton in that corollary only shows that the water in an equatorial canal surrounding the earth ought, when attracted by a luminary, to have a flux and reflux movement like that of the sea, and that "he gives neither the law nor the measure of the movement." Laplace adds that "the true explanation of the phenomenon is contained in Propositions 36 and 37 of the third book," which Mr. Airy does not refer to. There, in fact, Newton says expressly that "the sea is depressed at places  $90^\circ$  distant from the sun, and elevated both under the sun and in the opposite region," the same law applying, of course, to the moon.

Again, Mr. Airy asserts that, according to Laplace's theory, it will be low water under the moon. But in the *Mécanique Céleste* (vol. ii. p. 222) I meet with this sentence:—"If the ocean covered a spheroid of revolution, and if it experienced no resistance to its movements, the instant of high water would be that of the passage of the sun across the superior or inferior meridian." On what ground, therefore, is the opposite view attributed to Laplace? In the Essay on Tides and Waves in the *Encyclopædia Metropolitana* (arts. 109, &c.), Mr. Airy finds that it is low water under the moon by a method which he considers to be "equiva-

lent" to that of Laplace, and infers that Laplace should have come to the same conclusion. Without pretending to have formed an opinion respecting the correctness of this inference, I venture to say that the question admits of decision, apart from reasoning with mathematical symbols, from this single consideration. So long as the disturbing force tending to depress a *given* vertical column of the fluid is diminishing, or that tending to raise it is increasing, the length of the column is increasing; but as soon as the force ceases to increase, the raised column begins to descend by the force of terrestrial gravity. Accordingly there will be high water under the attracting body. It seems to me, after consideration of the terms employed in the latter part of Cor. 19, and especially of the meaning of the expression "*usque ad quadraturas*," that Newton intended to point out the kind of action I have just described, which is, in fact, the same as that from which he draws his conclusions in Prop. 36 of Book III. For these reasons I am of the opinion that the above-mentioned contradiction is due to fault or defect of principle in the method of solution the Astronomer Royal has adopted.

I think I am entitled to say that the solution of the problem of the tides given in this communication, which differs materially from that of Laplace, is the only one hitherto proposed that is strictly based on the necessary principles of hydrodynamics.

Cambridge, December 17, 1869.

## VI. *On the Motions of certain Liquids on the Surface of Water.*

By CHARLES TOMLINSON, F.R.S.\*

1. UPWARDS of thirty years ago I published<sup>1</sup> an account of some experiments on the action of vapours on films; and more than eight years ago some further results were given in these pages<sup>2</sup>. For the sake of convenient reference I will briefly recapitulate some of the leading facts.

*a.* If a drop of ether, or a bit of sponge tied over the end of a glass rod and dipped into ether, be held over the surface of clean water, the surface just below the ether will be depressed so as to form a cup-shaped cavity.

*b.* If the water be dusted over with lycopodium powder and the ether then held over the surface, the powder will be powerfully repelled from the cavity.

*c.* If the water be first covered with a film of oil, or turpentine varnish, &c., and the ether-sponge be held over it, very fine displays of Newton's rings will be produced.

\* Communicated by the Author.

<sup>1</sup> Student's Manual of Natural Philosophy (London, 1838), pp. 545 to 549, and pp. 553 to 555.

<sup>2</sup> Phil. Mag. for August 1861.

*d.* If about seven or eight parts of water be shaken up with one part of ether, the experiments *a, b, c* can no longer be performed on the surface of the resulting solution; but if this solution be boiled and filtered and left till cold, the results *a, b, c* can be obtained on its surface as before.

2. Experiments of this kind were performed by means of the vapours of various volatile bodies; but a very remarkable difference appeared in the behaviour of the vapour, according, as it seemed to me, as it had a strong attraction for the water, or for the substance of the film. In the one case the vapour would thin out and even disperse the film; in the other it would gather it up, thicken it, and deprive it of colour. For example:—

*e.* The ether-sponge was held over a film of oil of lavender; the latter was scattered and dispersed. A sponge dipped in benzole was now held over the surface, when the parts of the film became gathered together and thickened. In like manner, a film of oil of peppermint was scattered and dispersed by ether-vapour; but when the turpentine-sponge was held over the surface, the scattered parts sailed up to it and, being thus gathered together, formed a number of thickening lenticules. Other examples of this action will be given hereafter (18).

*f.* A drop of creosote on the surface of water will sometimes split up into a number of separate lenticular masses, sometimes remain as one such mass—each lens having motions of vibration and translation, and so wasting away.

3. Now in such a case as this (*f*), as the water approached the limit of saturation, the motions of the creosote became slower, and ceased altogether when saturation was fully attained, just as, in the case of the ether repulsions, the experiments *a, b, c* could not be performed on a saturated solution (as in *d*).

4. Hence these effects seem to arise out of that kind of adhesion which, overcoming cohesion, constitutes *solution*, and, when the two forces are in equilibrium, *saturation*. The strong attraction or adhesion of water for ether-vapour, and the appearance of the latter on the surface as a disk of liquid ether, combined with the expansive force and weight of the ether-vapour, apparently explained such results as those given under *a, b, c*; while the motions of the creosote (*f*) seemed evidently to come under the phenomena of solution. When, for example, a quantity of water had been saturated with creosote, so that all further motion was suspended, since solution was complete, if the quantity of water were increased, the motions set in again, and that without increasing the superficial area of the water; so also if the solvent power of the water was increased, as by the addition of a drop of acetic acid, the motions of the creosote were renewed.



In my first paper on the cohesion-figures of liquids<sup>3</sup>, an experiment is described in which creosote was placed on the surface of water in two similar glasses; but one of them contained one ounce of distilled water, and the other two ounces. The first drop of creosote lasted seven minutes on each surface, the second drop 20 minutes on one ounce of water,  $12\frac{1}{2}$  minutes on the two ounces; the third drop had not disappeared from the first glass after 135 minutes; but from the second glass it disappeared in 25 minutes, and even a fourth drop was disposed of.

5. Professor Van der Mensbrugghe<sup>4</sup> takes great pains to show that in the experiments *a, b, c* the ether-vapour streaming down upon the water on the film exerts no mechanical action by its weight. I only referred incidentally to this action, and remarked that a stream of carbonic acid gas from a gas-bottle will, by its mechanical action, produce iridescent rings on some films. The chief action seemed to me to be, as already stated (4), the strong adhesion existing between the water and the ether. The Professor explains the experiments *a* to *f*, as well as the varied phenomena of cohesion-figures, on the principle of surface-tension; and yet, before surface-tension can act in the way described, there must be adhesion. Of course he admits as much, and distinguishes between effects due to various degrees of this force which I have so much insisted on. For example, in § 51 of his memoir, in reference to some experiments by the Abbé Mann<sup>5</sup>, in which whale-oil spreads on the surface of boiling water, while olive-oil and linseed-oil refuse to do so, he says, "This arises most probably from the fact that the first liquid adheres much more strongly to the water than the other two oils, and can thus spread under the action of a force inadequate to the extension of the other two." And again, § 52, with reference to the formation of cohesion-figures, he says:—"It is evident that the different oils deposited on pure water cannot produce the same figures, since the phenomenon depends at once on surface-tension and on the amount of adhesion of the substances employed. Hence it is also clear that the same oil may produce different cohesion-figures when placed successively on clean water, mercury, sulphuric acid, &c. This explains perfectly the numerous varieties of figures described by Mr. Tomlinson."

6. Connected with these surface-motions is an experiment by Prevost<sup>6</sup>, and one also by Macquer<sup>7</sup>, of which I took note many

<sup>3</sup> Phil. Mag. for October 1861.

<sup>4</sup> "Sur la Tension superficielle des Liquides," *Mém. de l'Acad. Roy. des Sciences &c. de Belgique*, vol. xxxiv. 1869.

<sup>5</sup> *Mém. de l'Acad. de Bruxelles*, 1780, vol. ii. p. 257.

<sup>6</sup> *Ann. de Chim. et de Phys.* vol. iv. p. 31.

<sup>7</sup> *Dict. de Ch.* art. "Verre ardent."



years ago, but never could explain until I had read Professor Van der Mensbrugghe's memoir. Prevost noticed that if a red-hot iron wire or poker be presented obliquely to the end of a disk of tinfoil floating on water, the disk will be repelled; but if the hot wire be held vertically over the disk, it does not move. By directing the focus of a burning-glass on the disk, it is also repelled. The water must be perfectly clean, or the experiment will fail; simply touching it with the finger is sufficient to prevent the result<sup>8</sup>.

I had fancied that the red-hot wire diminished the adhesion of the surface locally, and during the expansion the disk got shifted to a cooler part, where the adhesion was stronger; but this could not apply to Macquer's observation, that gold spins when melted, and that minute fragments of grit on its surface are powerfully repelled. When the metal is fused by the action of a large lens, fine powder on the surface is dispersed as if by a powerful blast of the breath<sup>9</sup>.

7. I proceed now to describe the method by which Professor Van der Mensbrugghe gathers up the various facts described, together with many others that will be indicated, and presents them in harmonious relation to each other.

8. Admitting the surface-tension of liquids to be a *vera causa*, his first general proposition is as follows:—

Let  $a$  and  $b$  be the respective surface-tensions of two liquids, A and B, of which the second is supposed to be very volatile and more or less soluble in the first, or, at least, capable of adhering to it; pour the liquid A into a large perfectly clean capsule, then take up (by means of a capillary tube, for example) a small quantity of the liquid B, and hold it vertically over and very near to the surface of A; then, if  $a$  be notably superior to  $b$ , the subjacent layer will immediately move towards the edge of the capsule and a circular depression will be produced, the more decided in proportion as  $a$  exceeds  $b$ ; but if these quantities scarcely differ from each other, there will be no movement on the surface of A; if, on the contrary,  $b$  is greater than  $a$ , there will be developed a centripetal current; that is to say, the part situate beneath the tube will attract towards itself the neighbouring superficial portions.

9. In fact, if the vapour of B be more or less soluble in A, or remain adherent to it, and if, moreover,  $a$  be very superior to  $b$ ,

<sup>8</sup> Reference may also be made to P. Wilson's "Account of certain Motions which small lighted Wicks acquire when swimming in a basin of oil," Edin. Trans. vol. iv.

<sup>9</sup> The surface-tension of metals in a state of fusion would form a good subject of inquiry for any one who has ample means at his disposal. Results of great value might be expected from it.

the surface-tension of that part of the liquid immediately below the tube becomes less than on the surrounding portions, and these exert on that part a traction which is made evident by the apparent repulsion of the particles of powder floating on A; or, by this same force of traction, the superficial layer drags away the molecules situate below the tube and thus produces a depression. If this force be sufficiently energetic, waves are propagated on the surface of the liquid. When, on the contrary,  $b$  is greater than  $a$ , the subjacent portion of A acquires a greater surface-tension and attracts the surrounding layer, unless the resistance be too great.

10. Let A be distilled water at  $20^{\circ}$  C., when the tension  $a = 7.3$  milligrammes per millimetre of length; B ether;  $b = 1.88$ ;  $\frac{a-b}{a} = 0.74$ . When the interior diameter of the ether-tube is

less than a millimetre, the depression on the surface of the water is well marked; with a tube 2 millims. in diameter the concavity is from 4 to 6 centims. wide, and the particles floating on the surface are powerfully repelled towards the edge of the vessel. If the ether-sponge (1  $a$ ) or a finger wet with ether be held over the water, the effect is still more energetic, and a system of waves is propagated on the surface of the water. If the water be in the form of a stratum not more than 2 millims. thick, the part below the sponge will fly open and leave the portion of the bottom of the vessel thus exposed quite dry so long as the vapour of ether pours down<sup>10</sup>. If the water be made to rest on the surface of pure clean mercury, a much thicker stratum may be used; the ether-sponge will act at considerable distances, producing sensible motions in the well-defined perfectly circular iris of water. That vapour of ether will act at a distance and produce motions on the surface of water, was pointed out by B. Prevost in his controversy with Carradori, noticed in my former paper<sup>11</sup>. Fragments of gold leaf or of tinfoil floating on water became agitated as soon as a capsule of ether was placed at some height over the surface.

11. It will be understood that the above experiments cannot be performed if the water be previously saturated with ether, or if the water be soiled with the finger or otherwise, because in such cases the surface-tension or contractile force of A becomes

<sup>10</sup> Draparnaud (*Ann. de Chim.* vol. xlvii. p. 303) noticed that a thin layer of water at the bottom of a capsule flies open when touched by a drop of alcohol at the end of a glass rod, leaving the bottom of the vessel uncovered. The water slowly closes in again as the alcohol evaporates. The bottom of the capsule should not be convex. Carradori (*Mem. Soc. Ital.* vol. xii. p. 94) says this is an old observation, and refers to *Ann. di Ch. di Paria*, vols. xviii. & xix.

<sup>11</sup> *Phil. Mag.* for December 1869.

greatly reduced. The mere contact of the finger with the surface is sufficient to lower the tension from 7·3 to 4·5. The addition of 1 per cent. by weight of soap to the water lowers its tension to about 3, so that on such a surface the effects described cannot be produced.

12. Similar effects to those obtained with ether may be observed with alcohol, only less marked, since the difference  $a - b$  is actually less. Wood-spirit, acetic ether, paraffin oil, &c. exert also about the same degree of energy as alcohol. With turpentine the effects are still less marked, that liquid being much less volatile than ether or alcohol, and but little soluble in water.

13. When A is olive-oil ( $a = 3\cdot5$ ) and B ether,  $\frac{a-b}{a} = 0\cdot462$ , there is motion on the surface, as is evident from the apparent repulsion of dust-particles as soon as the ether-tube is adjusted. If the oil be in the form of a layer about 1 millim. thick, the approach of the ether will cause it to fly open (10). With olive-oil and alcohol,  $a - b = 1$ ,  $\frac{a-b}{a} = 0\cdot29$ , a similar effect is produced, although the difference between the tensions of the two liquids is only 1; whereas vapour of ether acting on a mixture of seven parts of water and one of ether produced no effect, although in such case  $a - b = 2\cdot12$ . The author explains this apparent anomaly by referring, first, to the difficulty vapour of ether has in dissolving in such a mixture, whilst alcohol is very soluble in the oil; and secondly, the *superficial viscosity* (or the greater or less difficulty the superficial molecules have in being displaced) is greater in the case of the ether solution than in that of oil of olives. A solution of carbonate of soda ( $a = 4\cdot28$ ), according to Plateau<sup>12</sup>, has a great superficial viscosity, and on this account

<sup>12</sup> See Phil. Mag. December 1869. The solutions used by Plateau in his ingenious experiments for determining the superficial viscosity were, of course, only saturated. In supersaturated solutions the superficial viscosity is one of the most marked features. As soon as the flask is closed or covered, the surface of the solution becomes quite viscous; and this increases during the cooling—so that, on gently shaking or inclining the flask, the superficial layer breaks up and partially mingles with the lower strata, like a strong syrup mingling with water. When a long and wide tube containing a strong saline solution is watched during the cooling, it is seen that the superficial layer is constantly being dissolved and as constantly being renewed, the one by contact with the hot liquid beneath, which causes the viscous layer to descend in saccharine-looking streams, the other by contact with the outer air, into which the surface-layer evaporates and becomes viscid preparatory to crystallization. If the tube remain uncovered, crystals begin to form on the surface, the air supplying the nuclei. If covered, in time the solution will cool down without depositing crystals, and the surface will long remain viscid. In my "Experimental Examination of the so-called Storm-glass" (Phil. Mag. for August 1863), many of the phenomena which accompany the cooling of saline solutions are given.

the ether-vapour has scarcely any action on its surface ; while there is no action at all with a solution of one part of saponine in sixty parts of water, although  $a=4.67$ , the viscosity of the surface of this solution being very great. On such a surface a drop of turpentine spreads almost as slowly as upon a solid surface.

14. A, carbonic disulphide( $a=3.57$ ); B, alcohol ;  $\frac{a-b}{a} = 0.30$ .

In this case the observation is difficult, because the particles floating on A are being constantly displaced by spontaneous movements, arising from variations in tension at different parts of the surface consequent on the evaporation of A (clogged as it is by the powder dusted on) being unequal. This produces a lowering of the temperature, the effect of which is to increase the superficial tension of a liquid in proportion as such reduction is great. It was evident, however, that the alcohol-tube did act in producing the apparent repulsion of the powder.

15. A, absolute alcohol ; B, carbonic disulphide;  $\frac{a-b}{a} = -0.30$ .

Since the difference in the tensions of the two liquids is negative, the motion on the part of A subjected to the vapour of B is no longer centrifugal, but centripetal. The alcohol was in a capsule of from 20 to 25 centims. diameter, and on the surface were dusted some light particles of bran ; then holding very near the surface a tube of not more than 1 millim. diameter containing a small drop of the disulphide, particles at the distance of from 4 to 5 millims. floated up and remained stationary under the tube. In fact the vapour of the disulphide condensing on the surface of the alcohol augments somewhat its tension, and thus produces an attractive effect on the neighbouring particles.

16. If in this experiment a tube of 3 or 4 millims. be used, the particles are first attracted near to the drop and then immediately repelled. The author explains this by supposing that the condensation of the vapour slightly augments the temperature of the subjacent portion, while evaporation is diminished there by the presence of the tube and of the disulphide vapour. But the surrounding portions of the alcoholic surface evaporate freely, and so acquire an exaltation of tension more than sufficient to counterbalance the increased tension of that part of the surface just below the tube. In fact it can be shown that if evaporation be interrupted at one part of the surface of the alcohol, the neighbouring parts pull upon it, as is evident from the apparent repulsion of the particles on the surface.

17. A, carbonic disulphide ; B, oil of turpentine ;  $\frac{a-b}{a} = 0.19$ .



No effect, probably because the vapour of the disulphide being heavier than that of the oil prevents it from being deposited on the surface of A, and so renders any variation in tension impossible. When, however, the vapour of the disulphide acts on a surface of turpentine dusted over with lycopodium, the particles near the end of the tube move up to it and produce a centripetal current.

18. I will now give some further experiments of my own, illustrating on a large scale the formation of centrifugal and centripetal currents. A bit of sponge tied over the end of a glass rod (1 *a*) was used to convey the vapour, a separate sponge being used for each liquid.

Vapour of chloroform produces powerful centripetal currents in the films of various oils &c. on the surface of water. Many years ago I observed that a large film of black Japan varnish on the surface of water contained in a dinner-plate was singularly acted on by the chloroform-sponge. The vapour first converted it into a multitude of minute globules, which then moved up to the sponge as to a common centre of attraction.

A similar effect is produced by the action of the ammonia-sponge on a film of oil of tar (*Ol. picis*). No sooner is the sponge held over the film than it separates into distinct portions, and each portion gathers itself up into a very convex lens, which reflecting the light, the appearance is as if the surface were all at once studded with eyes. The effect is very singular and striking. Chloroform slightly repels this film.

*Oil-of-turpentine Film.*—Chloroform powerfully attracts and gathers it together. It does the same with films of the oils of organum, rosemary, lavender, and many others. But it often happens that, when chloroform has gathered up the film, the ammonia-sponge will spread it out sufficiently thin to form iridescent colours. Such is the case with the films of oils of lavender, amber, &c. So also benzole will thicken and attract a film, while ether will scatter and repel it. The naphtha-sponge acts centripetally on the oil-of-sassafras film, while on a film of oil of pepper it produces a remarkable swinging to-and-fro motion. Acetic acid, in which oil of lavender is soluble, acts by its vapour upon a film of the oil in a very marked manner, attracting it and gathering it up into little disks. This film is also very sensitive to the ether-sponge, even when held at a great distance above. The oil-of-aniseed film is dispersed by ether and brought together by ammonia. A film of oil of turpentine is first repelled and then attracted by the ammonia-sponge. The oil-of-rosemary film is gathered up by the vapour of carbonic disulphide. Ether disperses a film of oil of peppermint; vapour of turpentine brings it together.



19. Effects of this kind I endeavoured to explain by supposing the liquids, in passing into vapour, were endowed with a repulsive force; but when such vapours condensed on the film and became soluble in it, attraction set in accompanied by an increase of cohesion. I fully admit the great superiority of the theory which accounts for and explains these varied effects by variations in the surface-tension. Some of the phenomena just given (18), and others to which I shall afterwards refer (28) (32), always puzzled me until I saw what a vast number of analogous phenomena were generalized by the theory of the surface-tension of liquids.

20. There is one of my results which Professor Van der Mensbrugghe explains in a manner that does not seem to me altogether to meet the phenomena. I state two effects: (1) that the oil-of-turpentine sponge produces beautiful iridescent rings when held over a film of oil of turpentine; (2) that when the film or cohesion-figure has passed into what I call its fourth phase<sup>13</sup>, and a lace-like pattern is left on the surface of the water, if a drop of oil of turpentine be held over the surface the parts will gather together and display colours. On referring to my note-book, I find the first effect also entered for the oils of rosemary and mint, and also for balsam of Tolu; that is, a film of one of these would display iridescent rings by the action of its own vapour. These experiments were first made in bright summer weather, which is so favourable to all the camphor and other surface-motions. On attempting to repeat them recently I failed; that is, I got no repulsion or thinning of the film by the action of its own vapour upon it. When the film had become attenuated by evaporation, I obtained the iridescent effects by the action of the turpentine-sponge. This arises, according to the theory, from an increase in thickness consequent on the condensation of vapour; and as the tension of the water so covered diminishes up to a certain limit in proportion as the thickness of the film is increased, it will be understood how, as the tension becomes less in the coloured portion, the surrounding parts remove this from the centre until the white of the first order is again developed at this centre. The rings then remain stationary, or even become slightly compacted by virtue of the tension of the central part. The condensation of a fresh portion of vapour only slightly changes the aspect and diameter of the rings, so that the central white remains. When the source of vapour is removed, the rings disappear and the white tint covers the whole film.

<sup>13</sup> The drop of turpentine on water forms (1) a well-defined film, with a double row of bosses of unequal size just within the edge; (2) the film displays iridescent colours; (3) it becomes perforated with small holes; (4) it forms a delicate network.

21. The spreading of a drop of a liquid upon the surface of another liquid is dependent on the conditions enunciated by Professor Van der Mensbrugghe in the following proposition:—

When on the surface of a liquid, A, whose tension =  $a$ , we deposit a drop of a liquid, B, whose tension,  $b$ , is notably smaller than  $a$ , this drop usually spreads into a thin film with a display of beautiful colours. But if  $b$  is superior to, or scarcely inferior to  $a$ , the drop, instead of spreading, assumes and maintains a lenticular form. Hence it follows that if a liquid, B, is capable of spreading on another liquid, A, it is not possible that A should spread upon B.

22. In order to show the superior tension of that portion of the surface not covered by the film, the author resorted to the ingenious contrivance of the silken coil described in my former paper<sup>14</sup>. The action of ether and also of camphor within and without the coil has already been given<sup>15</sup>. One more example will repay perusal.

A, distilled water; B, solution of at least one part of Marseilles soap in forty parts of water ( $b=2.83$ );  $\frac{a-b}{a}=0.61$ .

As soon as a drop of the solution touches the liquid surface within the coil, this immediately assumes the circular form; but it does not rebound as in the case of ether, because the soapy solution, not being very volatile, does not increase the contractile force by the cold incident on evaporation; and the oily matter of the soap greatly diminishes the tension of that part of the water on which the drop spreads. It is curious to remark that the floating disk of liquid bounded by the coil moves over the surface of the water, and even rotates, without changing its circular form. These effects are thus accounted for:—If the boundary limited by the coil stopped all communication between the lamina of soapy water and the surface of the pure water, it would be tended equally in all directions by the external liquid layer, and so would remain in equilibrium; but along this boundary there exists generally one or several points where it is possible for the soap-disk to communicate with the pure water outside the coil; and it is here that a small portion of the soap-solution escapes from the coil in a certain direction, and neutralizes in this direction the external traction which would otherwise act on the coil at these points. Consequently the forces to which the system is subject are not everywhere and in all directions equal among themselves, so that equilibrium cannot be maintained. It is easy to conceive that the sum of these forces may be replaced by a single resultant, and then the coil would undergo a movement of translation; or by a couple, when we should

<sup>14</sup> Phil. Mag. for December 1869, § 31.

<sup>15</sup> Ibid. §§ 32 & 35.

get a movement of rotation ; or by one single force and a couple, when the soapy disk would be displaced in turning upon itself. These motions do not continue long, since the soapy solution gradually invades the surface of pure water external to the coil, and so diminishes greatly its tension.

23. It has already been stated (20) that the tension of water covered with a film of turpentine diminishes up to a certain limit in proportion as the film becomes thick. Water covered with a film too thin to give a uniform white tint of the first order has a tension equal to about 5 ; after a second drop of turpentine has been deposited, the contractile force is only 4.71, after a third 4.12, after a fourth 3.7, and after a fifth 3.43. As the tension of turpentine is only 2.9, we have a sufficient reason why several drops of turpentine in succession spread upon the surface of water. As soon as the contractile force of the water is reduced to 3, the drops of turpentine no longer spread, but assume the lenticular form. I may remark, however, that the phenomena of cohesion-figures are not well exhibited after the deposit of the first drop. Some oils (that of lavender, for example) will perform their evolutions on the film of a second drop, but a third drop remains as a film. It also happens, with newly distilled oils especially, that the second drop remains for a long time in the spheroidal state on a film of the first, from which it is separated by a cushion of its own vapour. I beg to refer to a note on this subject<sup>16</sup>.

24. It will be readily understood that the great difference in tension between water and the oils causes the latter to spread rapidly on the surface of the former, while they maintain the lenticular form if the water be covered with even an imperceptible film of greasy matter, since in such case the tension of the water becomes diminished. It is a difficult question to determine how the lenticular form can constitute a figure of equilibrium when the tension of the water on which the oil-lens rests is still greater than that of the oil. Professor Van der Mensbrugghe takes it for granted, although I very much doubt the fact (25), that at the surface of separation of the two liquids there is greater or less adhesion, which must diminish the contractile force of each liquid, so that a resultant tension prevails over such surface which is neither that of the water nor that of the oil. Hence he arrives at the general condition of equilibrium of a liquid lens resting on the surface of another liquid. Let  $t$ ,  $t'$ ,  $t''$  be the respective tensions of the surrounding liquid, of the upper face of the lens, and of the surface of separation of the two liquids, and  $\alpha$ ,  $\alpha'$ ,  $\alpha''$  the angles formed with the horizon at one point of the edge of the lens by tangents to the sec-

<sup>16</sup> Phil Mag. for December 1863.

tions determined by the meridional plane of the lens in the external liquid surface, and in the superior and inferior zones of the floating mass. We then get, in the case of the spreading of the drop, the condition

$$t \cos \alpha > t' \cos \alpha' + t'' \cos \alpha'';$$

and in the case of equilibrium of the liquid lens, the relation

$$t \cos \alpha = t' \cos \alpha' + t'' \cos \alpha''.$$

But these formulæ also show that  $t$  may considerably exceed  $t'$  without the extension of the drop into a film being possible, and that, if the spreading does take place, it is feeble in proportion as  $t''$  becomes greater—that is to say, as the two liquids brought into contact have less mutual adhesion. Unfortunately the difficulties of obtaining direct measurements of  $\alpha$ ,  $\alpha'$ ,  $\alpha''$ , and especially of  $t''$ , are opposed to the exact valuation of the limit beyond which extension becomes impossible.

25. It would be very desirable to ascertain the value of  $t''$ , or the tension of the two liquids at the surface of separation—whether in fact there were any, and what amount of, adhesion between the two surfaces, and how far this adhesion modified the tension. I have already in the former paper<sup>17</sup> insisted somewhat on an experiment in which the motions of camphor and benzoic acid on the surface of water are not interrupted by the presence of a lenticular mass of a newly distilled essential oil on the surface. The camphor will skate through it with great vigour, cutting it up into smaller lenses, and is apparently in no way impeded by its presence. Hence it may be argued there can be little or no adhesion between the oil and the surface of the water, or the motions of the camphor would be arrested, as is so decidedly the case when a fixed oil spreads out into a film.

26. As this is a point of importance, I may perhaps be permitted to refer to some results obtained during the present year as to the action of oils in inducing crystallization in supersaturated saline solutions, of which those of sodic sulphate may be taken as the type. If we deposit on the surface of such a solution a drop of oil, fixed or volatile, one of two things will happen; it will either spread out into a film, or it will form a lens. In the one case, where a film is produced, this film acts as a powerful nucleus, finely shaped crystals moulding themselves, as it were, on its under surface, and then falling away, to make room for others. In the other case, not only is there no crystallization as soon as the drop is deposited and the lens formed, but it may be left for weeks and months, through all the variations of atmo-

<sup>17</sup> Phil. Mag. for December 1869, §§ 28 & 39.



spheric temperature, resting on (I do not say in contact with) a highly supersaturated and consequently very sensitive solution, without any separation of the salt arising from nucleate action. Nay, more, during all this time the flask containing the solution may be shaken so as to disperse the oil in globules through the solution, and yet there is no crystallization. So far as their action as nuclei depends, they might as well be outside the flask. In such a case, as it seems to me, each globule of oil does not adhere to the solution, although this may completely surround it when the flask is shaken, but maintains its own surface-tension, while that portion of the solution which moulds itself upon it and forms, as it were, a new surface, thus acquires and preserves its own peculiar surface-tension; so that in fact the oil-globule and the solution are not in contact at all. The extreme sensitiveness of the solution to the action of nuclei renders this view probable. We have only to convert an oil-globule into a film, which may be done by a peculiar jerk of the flask, whereby a few globules flatten against the side and spread, when in an instant, as if by a flash of light, the whole solution becomes solid. By thus converting an oil-globule into a film we ensure contact; and contact is necessary to convert the non-nucleate oil-lens or globule into a nucleate oil-film. Now this condition of film is precisely that which we get when a glass rod, for example, made chemically clean and therefore non-nucleate, is rendered powerfully nucleate by simply drawing it through the hand, whereby it becomes tainted with a film of greasy matter. In like manner a glass rod smeared with oil is a nucleus; but if the oil be made to roll up into globules, neither the rod nor the oil-globules act as nuclei<sup>18</sup>.

27. A, mercury ( $a=49\cdot1$ ); B, ether;  $\frac{a-b}{a} = 0\cdot96$ . Ether spreads rapidly on the surface of mercury, and then resolves itself into a multitude of small lenses. On repeating the experiment a few times, the surface of the metal became covered with a thin film apparently of greasy matter, arising, as Professor Van der Mensbrugghe suggests, from the ether; but may it not rather have originated in the mercury, which is very difficult to maintain chemically clean? Alcohol, benzole, wood-naphtha, and the oils spread rapidly on the surface of mercury, as would naturally be supposed from the great differences between their tension and that of mercury. But a difficulty arises in the case of water, which does not spread on mercury, although its tension is six times less than that of mercury. A drop of distilled water on

<sup>18</sup> The details of this subject, including the action of various liquids besides oils, and the peculiar mode of manipulating with the solutions, are reserved for a separate notice.



such a surface maintains the form of a spherical segment not greatly differing from a hemisphere; but the water has very little adhesion for mercury, so that the tension  $t''$  of the surface of separation of the two bodies has a value very nearly that of the tension  $t$  of the mercury. On the other hand,  $\alpha$  and  $\alpha''$  have no value, on account of the great density of the metal. The equation therefore is only possible when the  $\cos \alpha'$  is very small—that is to say, when  $\alpha'$  scarcely differs from a right angle and the drop is sensibly a hemisphere. The same equation shows that when by any means we diminish  $t'$  or  $t''$ , the spreading immediately follows.  $t'$  may be diminished by touching the surface of the segment with a morsel of soap; the soap-water covers the free surface of the small liquid mass, and in a few seconds the mass itself spreads, since soap-water has a much smaller tension than pure water. The same result may be obtained by touching the surface of the segment with a speck of oil.

28. I observed many years ago that a film of very dilute sulphuric acid on the surface of mercury was gathered up into a lens by the action of the vapour of absolute alcohol, or of ether, held over it. If a thin stratum of water adhere to a clean glass plate, the vapour of ether or of alcohol will make it fly open, as already noticed (10). Creosote by contact will do the same. These apparent repulsions between two liquids, of which so many examples have long been known, are simple effects of surface-tension. It is remarkable that so long ago as the year 1855 Professor Thomson of Belfast<sup>19</sup>, brought before the British Association at Glasgow the phenomena of “weeping,” or “tears in the wine-glass,” and connected them with surface-tension. He says: “The more watery portions of the entire surface having more tension than those which are more alcoholic, drag the latter briskly away, sometimes even so as to form a horizontal ring of liquor high up round the interior of the vessel, and thicker than that by which the interior of the vessel was wetted. Then the tendency is for various parts of this ring or line to run together to those parts which happen to be most watery, and so there is no stable equilibrium for the parts to which the various portions of the liquid aggregate themselves, soon to be too heavy to be sustained, and so they fall down. In a wine-glass the thin film adhering to the glass must very quickly become more watery than the rest, by the evaporation of the alcohol being more rapid than that of the water.” Thus if a phial partly filled with wine & corked be shaken and then left to rest, no such motions are observed; but if air be withdrawn by means of a tube, and fresh

<sup>19</sup> Phil. Mag. S. 4. vol. x. p. 330, “On certain curious Motions observable at the Surface of Wine and other Alcoholic Liquids.” See also note <sup>21</sup>.

air capable of producing evaporation be introduced, the liquid film creeps up the interior with thick or viscid-looking pendent streams descending from it like a fringe from a curtain; the more watery portion draws itself away from the more alcoholic portion, as when a drop of alcohol is put into the centre of a thin stratum of water the water will retire (10), (28).

29. If the conditions be inverted and a drop of water be deposited on a thin stratum of ether or absolute alcohol, the drop assumes the form of a well-defined spherical segment, notwithstanding the great affinity existing between the two liquids. So also a stratum of olive-oil retreats before a drop of turpentine, cold water before hot, distilled water before soapy water, and so on. Invert these conditions, and there is no retreat of the liquid.

30. The phenomenon pointed out by M. Jeitteles in 1853<sup>20</sup> belongs to differences in surface-tension. When a liquid descends in a thin layer down the interior surface of a funnel or of the vessel which receives it, light particles floating on the surface rise sometimes many centimetres in a direction contrary to that of the current and describe within this current oval lines. The effect may be observed with water, saline and acid solutions, &c., but not with alcohol or ether. The conditions required for the exhibition of the phenomenon are a liquid of great surface-tension and an unclean vessel, or one more or less contaminated with greasy matter, as from the hands or an ordinary glass-cloth. Then, if a liquid with its surface-tension undiminished descend in a layer down the side of the funnel and come into contact with another portion of liquid whose tension has been diminished by contact with the greasy film, this portion will tend to obey the preponderating contractile force—that is to say, to form an ascending current. The current is not visible unless the beak of the funnel dip into the liquid in the vessel below; and the slower the motion the better the effect, since in such case the effects of the higher tension are less interfered with. That this explanation is the true one was proved by Professor Van der Mensbrugghe, who employed a chemically clean funnel, when the effect was no longer obtainable.

31. The various motions of different liquids on the surface of water which I have described from time to time, such as those of creosote<sup>21</sup>, eugenic acid<sup>22</sup>, and the displacing-power of some liquids over others<sup>23</sup> receive their proper solution in this principle of surface-tension<sup>24</sup>. While preparing the last-named

<sup>20</sup> *Zeitschrift für Naturwiss.* p. 445.

<sup>21</sup> See note 2.

<sup>22</sup> *Phil. Mag.* July 1864.

<sup>23</sup> *Ibid.* June 1867.

<sup>24</sup> There are some curious experiments described by M. Dardenne (*Bull. de la Soc. Roy. Bot. de Bruxelles*, vol. iii. Nos. 1 and 2). Happening to touch

paper, I noticed a curious series of motions with the film of oil of nutmegs that had been broken up by the action of creosote and exposed some time to the air. The oil formed a well-defined film with small bosses just within the edge; and the film almost completely covered the surface of the water in a shallow glass 3 inches in diameter. On placing a drop of creosote near the edge, it entered the film and cleared out a space for itself, although it did not vibrate as it usually does when alone on the surface. It gradually flattened; and as the film became thinner by evaporation and displayed fine iridescent colours, the creosote became active, broke up the film, and drove about the fragments. When, however, the creosote was placed first on the water and then the oil, the latter flashed into a film and stopped the motions of the creosote; but as the oil got thinner, the creosote disks became active and cut through the film with energy. One disk of creosote became attached to the edge of the glass and exerted its influence chiefly in a right line nearly to the opposite point. Portions of the film, now become almost powdery in texture, sailed up in gently curved lines to within one-tenth of an inch of the creosote, and then fell-in in two and two, *i. e.* one from either side of the creosote, and proceeded in a straight line nearly to the opposite point with all the order and regularity of a drill movement. This effect lasted about fifteen minutes, until the creosote had nearly disappeared. When the oil was redistilled, it formed a smaller film than before; a drop of creosote shut it up instantly. When the creosote was put on the water and then the newly distilled oil, the latter formed a lens.

32. There are many curious phenomena which I noted down during my study of cohesion-figures which could not well be

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with a chalky finger a pane of glass wet with condensed moisture, he noticed that as the streams descended stained by the chalky particles, some of these followed the descent, but others near the edge of the stream moved upwards. He then ground up a little oil with chalk or with a few drops of water, and on leading a gentle stream of water into it down an almost vertical plane, active contrary currents were obtained. In this case the pure water reaching the oily deposit, its excess of tension over the oily matter would tend to raise the superficial layer of oil and with it the solid particles suspended in it. It seems to me, however, that, in such cases as the above, other forces are at work as well as surface-tension. In referring to the phenomena of "tears in the wine-glass" as effects of a chemically clean surface (Phil. Mag. for October 1868), it is remarked that in the spaces between the tears will be seen an ascending current "which rises (1) by the adhesive action of capillarity, (2) by the formation of a back current in consequence of the downward flow of the tears, just as a backwater is formed at the place where two currents of a river meet." So also in endeavouring to explain the motions of eugenic acid (see note <sup>22</sup>), eddies are formed and an indraw such as are due to the operation of well-known forces apart from surface-tension.

explained by adhesion only, or by the resultant force of the adhesion of the surface and the cohesion of the oil. For example, a film of *Ol. Rutæ* sends out short processes from the edge, which sometimes split at the free end and become bifurcated with wide-spreading forks; or the free end may terminate in a boss, which is torn away and another formed. I have also observed a hole to open in the centre of the film, showing colour; after a time the colour would disappear and streamers set in directed towards the centre of the opening. Then the hole enlarged and the film separated into two or three well-defined disks, which remained quiet for a time, and at length put forth active streamers from the edge, which sailed away and disappeared. The splitting up and scattering over the surface on the part of some films I have endeavoured to represent in the case of creosote in the plate which accompanies one of my papers<sup>25</sup>; and the spasmodic actions which accompany separated portions of some other films are represented in the subsidiary figures *b, c, d* which accompany the engraving of the principal figure of oil of lavender<sup>26</sup>. The waving to-and-fro motion of the film of oil of bitter almonds and some other oils is very remarkable. In bright warm weather the energy of some oils on a large surface of water is striking. A disk of oil of cloves, for example, will set the whole surface vibrating after the manner of camphor pulsations<sup>27</sup>. When one drop of absolute alcohol was added to twenty drops of oil of cloves, and one drop of the solution placed on the surface of water, the disk that was formed shot out volleys of small globules with immense velocity, which covered the surface, and then all became quiet. In attempting to estimate the value of the cohesion-figure test by adding small proportions of the fixed oils to the more costly essential oils, an endless variety of phenomena was obtained, which, as it seemed to me, only required to be studied in order to render this test valuable. I refer to them now as affording good illustrations of variations in surface-tension; but it might be tedious if these examples were prolonged.

33. In conclusion, I must express my satisfaction that the vast number of phenomena described or alluded to in this and my former paper have been erected into a tasteful and harmonious structure by the Belgian physicist. I have contributed a good many bricks to the building; but it is a privilege to serve science, even though it be in the humble capacity of hod-carrier to a master bricklayer.

Highgate, N., Dec. 13, 1869.

<sup>25</sup> Phil. Mag. for June 1867, Plate IV.

<sup>26</sup> Phil. Mag. for October 1861, Pl. IV.

<sup>27</sup> Phil. Mag. for December 1869, § 26.



VII. *Note on some Propositions in the Theory of the Tides.*  
*By the Rev. T. K. ABBOTT, M.A., Fellow and Tutor of Trinity College, Dublin\*.*

**I**N the following paper it is proposed merely to give elementary proofs of the following propositions:—

I. That if there were no friction it would be low water under the moon.

II. That friction accelerates the times of high and low water.

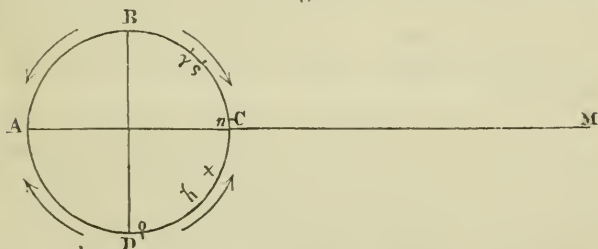
III. That, in addition to the oscillatory motion of the water, there is a constant current produced by the moon's action. And

IV. That the effect of friction is to increase the length of the day.

I. The theorem that if there were no friction it would be low water under the moon, is common to the theories of Newton and Laplace and Airy. That it is not mentioned in elementary treatises is, I suppose, owing to the circumstance that the proof is supposed to require a knowledge of the higher mathematics. The Astronomer Royal gave a simple proof of the theorem in the 'Astronomical Notices' for 1866; but as it is very indirect, and is open moreover to another objection presently to be mentioned, it does not make another attempt at simplification superfluous. The following proof is direct, and presupposes only the most elementary knowledge.

I suppose the moon to be fixed, and the earth rotating in the

Fig. 1.



direction A B C D, carrying the ocean with it. It is shown in elementary books (Galbraith and Haughton's 'Manual of Astronomy,' p. 66) that the tangential force alone need be considered, and that this acts in each quadrant in the direction indicated by the arrows, viz. always towards the line joining the centres of the moon and the earth.

Now in the course of one lunar day every particle of the ocean is subjected to precisely the same forces acting in the same order of succession and for the same periods, being accelerated for

\* Communicated by the Author.



about one quarter of a day, viz. while passing from B to C, then retarded for a quarter, from C to D, and so on. The variation in the amount of the force does not concern us, being the same for every particle.

This being so, it is obvious that those particles will be moving faster which have been for a longer time acted on by an accelerating force, and the velocity will be a maximum when the accelerating force has acted during its full period, viz. through one quadrant. On the other hand, those particles will be moving slower which have been longer acted on by a retarding force, and the absolute velocity will be a minimum when the retarding force has acted during its full period, or through one quadrant. The maximum velocity is therefore at A and C, the minimum at B and D.

Secondly, it is clear that the tide will be rising where each portion of water is moving faster than that just in advance of it, or, in other words, where water is flowing in faster than it flows out. Where this process has gone on for the maximum time, the tide will be highest. On the other hand, the tide will be falling where the water is moving slower than that in advance of it, or, in other words, is flowing out faster than it flows in. Where this has continued for the maximum time the tide is lowest.

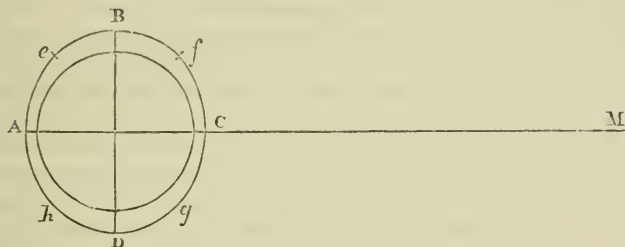
Now consider any point  $s$  in the quadrant B C. The water now passing  $s$  has been subject to an accelerating force during the whole time since it passed B, longer therefore than any particles behind it, as at  $r$ . It is therefore moving faster; and as the water in the space  $rs$  is thus flowing out faster than it flows in, the tide is falling. This is the case through the whole quadrant B C.

At C the force changes and becomes a retarding force. A particle at  $y$  has been subject to this retarding force longer than one behind it, as at  $x$ , and is therefore moving slower. Here, therefore, water is flowing in faster than it flows out, and the tide is rising; and this holds through the quadrant C D. What is said of these quadrants holds also of those opposite to them; the tide is falling all through D A and rising through A B. Hence it is highest at B and D, lowest at A and C. Where will the tide be falling fastest? Clearly where the difference of velocity between  $r$  and  $s$  is greatest, *i. e.* where the amount of force to which the water at  $s$  has been subject since it passed  $r$  is greatest—in other words, where the force is at its maximum, viz. at  $f$  (fig. 2),  $45^\circ$  from C. Similarly it will be rising fastest at that point in the quadrant C D where the force is greatest, viz. at  $g$ ,  $45^\circ$  from C.

On the whole, then, the water assumes the form of an ellipse, and as it is the earth that is rotating, this ellipse does not change

its absolute position except with the moon's monthly motion; only the water accompanying the rotating earth moves fastest at A and C, and is there shallowest: and slowest at B and D, and is there deepest. Relatively to the earth it is moving westward at B and D, eastward at A and C.

Fig. 2.



II. The theorem that the effect of friction is to accelerate the time of high and low water admits of an equally simple proof. As the water approaches C, the tangential force diminishes gradually to zero at C. Therefore it must have been equal to the force of friction at some point *n* (fig. 1), after which friction prevails and the velocity diminishes. It is therefore low water at *n*. Approaching D, the ocean is moving slower than the earth; therefore here friction tends to accelerate it, while the retarding force is decreasing to zero. The two forces, then, must be equal at some point *o*, after which the velocity again increases. It is high water therefore at *o*.

It is proper to observe that the preceding proof assumes (as does Mr. Airy's) that the ocean is carried round by the earth in its rotation. This amounts to supposing that it has not assumed a position of equilibrium\*.

Mr. Airy assumes further, that when the tide is rising fastest the water is flowing in from both sides. This is by no means evident. The rate of rise depends on the difference in velocity between two successive parts of the ocean, and this may be greater when the two velocities have the same sign than when they have different signs. Taking into consideration the rotation of the earth, the assumption amounts to this—that the tide is rising fastest where the velocity of the ocean is just equal to that of the earth. This is certainly not evident: in fact it would not be true if the tangential force did not decrease at the same rate on both sides of each of the four maxima. It ought not, therefore, to be assumed, but deduced.

III. We now come to the question whether, in addition to the

\* Mr. Airy indeed supposes, not only that the ocean has some movement of rotation, but that its mean movement is the same as the earth's.

oscillatory motion, there is any constant current produced by the moon's force. Now, as the circumstances about the four points of equilibrium are precisely similar as long as the ocean is supposed spherical, we may assume that the relative velocity of the water at equal distances from each of these points is the same, the points of no relative velocity being *e, f, g, h*. This is equivalent to supposing that the mean movement of rotation of the ocean does not differ *ab initio* from that of the earth. In this case, then, the retardation between *e* and *f* is exactly balanced by the acceleration between *f* and *g*, and similarly in the other two quadrants. But when the ocean has assumed the form of an ellipse this symmetry ceases. As the tangential force is, *cæteris paribus*, proportional to the distance of the particles attracted from the centre of the earth, it follows that the forces are greater from *e* to *f* and from *g* to *h* than from *f* to *g* and from *h* to *e*. Therefore the retardation in the former intervals is not completely compensated by the acceleration in the latter. Hence there is a balance of retardation, in other words, a continuous current, relatively to the earth, westward.

IV. Friction being taken into account, the effect of this is to retard the earth's rotation, or to lengthen the day.

### VIII. On Secondary Currents and a new Secondary Battery.

By JOHN PARNELL, M.A., F.R.A.S.\*

1. **A**S far as I am aware, no account has hitherto been published of the employment of alkaline solutions in the development of secondary currents; the investigation described in this paper was therefore undertaken for the purpose of ascertaining whether by this means effects more powerful than those previously observed could be obtained; and the results appear to be satisfactory.

2. The apparatus employed consisted of:—

(a) A Daniell cell as an exciting couple, connected with an ordinary commutator.

(b) The experimental secondary couple, which was formed of two plates of the same metal, one of which was folded round the other in a U-form. These two plates were separated from one another by pieces of ebonite at a distance apart of about 2 millims. The available surface of the inner plate was nearly 14 square centimetres.

(c) A Morse key, by which one pole of the secondary couple could be rapidly detached from the exciting cell and thrown into communication with

\* Communicated by the Author.

(d) A Thomson's astatic reflecting galvanometer, whose internal resistance was rather less than  $\cdot 6$  B.A. unit. The deflections were read off in the usual manner, on a scale which was so graduated that 300 divisions occupied a space of 19 centims. The image of the slit appeared as a band of light 4 millims. broad, the position of one edge of which, sharply defined, gave the index-reading. By using a lens sufficiently large for the scale to be viewed by both eyes at once, the error of observation did not exceed  $\cdot 5$  of one division, or, in the deflections generally obtained,  $\cdot 3$  per cent. of each reading. Attached to the galvanometer was a shunt, by which 9 per cent. or 99 per cent. of the current could be diverted at pleasure from the galvanometer-coils. The directing magnetic bar was placed at the top of its support, and with the same orientation as the upper needle.

The index-readings taken were in all cases those corresponding to the extreme swing of the galvanometer-needle.

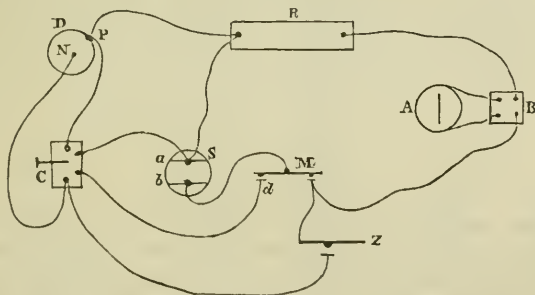
(e) A set of resistance-coils from 1 up to 13,000 B.A. units.

(f) A key, by means of which the current from the Daniell cell could from time to time be sent to the galvanometer through the same resistance-coils as the secondary current.

(g) Other apparatus, mentioned in the account of its employment.

3. The connexions are shown in the accompanying figure (fig. 1), where A is the galvanometer, B the shunt, C the com-

Fig. 1.



mutator, D the Daniell cell (of which P and N are the poles), M the Morse key (of which  $d$  is the anvil on which the spring presses the lever), R the resistance-coils, S the secondary couple (of which  $a$  and  $b$  are the plates), and Z the key (§ 2 f). When the commutator is so turned that  $a$  is connected with P, N is in metallic communication with  $b$  through the lever of the Morse key, and the secondary couple is being excited; but on depressing the lever,  $b$  is shunted from N to the galvanometer, and the secondary circuit is completed through the resistance-coils to  $a$ .



When the commutator is turned off, by depressing the key Z the Daniell current is sent through the resistance-coils to the galvanometer.

4. The *modus operandi* will now be quite apparent: the index having been carefully adjusted to zero, the commutator was turned so as to excite the secondary couple, and at the expiration of twenty seconds, the interval having been taken by the ticks of a clock beating dead seconds, the Morse key was depressed, the index-reading observed, the commutator turned off, and a short circuit made to exhaust the secondary couple before another experiment was made. This process was repeated in each case till five results had been obtained, the mean of which was recorded. After the first, third, and fifth experiment the deflection of the galvanometer by the Daniell cell was observed and the mean of these results noted. The ratio of the secondary to the Daniell mean, multiplied by  $10^3$ , is the figure of merit recorded in this paper, and gives the value of the couple under examination, that of the Daniell cell being 1000. During a set of five consecutive experiments, the index-reading due to the Daniell cell never differed from the mean by more than one division of the scale. The following instance of a set of experiments, taken at random from the many hundreds which have been made on this subject, will give a fair notion of the results obtained (Table I.):—

TABLE I.

No. of experiment.	Secondary readings.	Daniell readings.	Ratio of means, or figure of merit.
1. . . .	157	176	
2. . . .	158		
3. . . .	157	176	
4. . . .	158		
5. . . .	157	175	
Means . .	157.4	175.6	896

Unless otherwise mentioned, throughout the whole course of the experiments herein described 9 per cent. of the currents was shunted from the galvanometer; and the resistance employed was 7000 B.A. units, in comparison with which all the other external resistance, and *à fortiori* any accidental variations in it, vanished.

5. Some preliminary experiments showed that copper plates in a solution of carbonate of sodium formed a powerful secondary combination; and, to see what advantage it possessed relatively to known secondary arrangements, it was compared with platinum, palladium, and lead in dilute sulphuric acid (1 part acid, 6 parts water). The specific gravity of the solution of the carbonate, as well as of all the other alkaline solutions mentioned



in this paper, was 1050, that of water being 1000. The following results were obtained\* :—

TABLE II.

		Figures of merit.
Daniell	. . .	1000
Cu	}	853
Sod. carb.		
Pt	}	772
Dil. sulph. ac.		
Pd	}	732
Dil. sulph. ac.		
Pb	}	306
Dil. sulph. ac.		

6. When the copper plates were clean and polished, the figure of merit was far below that mentioned above; but the more frequently the plates were excited, and the more corroded they became, the stronger was the current produced. Copper plate as it comes from the rollers will, under the influence of powerful electric currents, very quickly acquire a condition of surface favourable to the development of secondary currents; and that condition, once acquired, is apparently always retained. Indeed the highest result obtained with any experimental couple was given by a pair of copper plates whose surfaces appeared to be completely covered with crystals of carbonate of copper, the solution in which they had been placed having evaporated.

7. Other alkaline solutions were then tried, viz. of the hydrates of sodium and potassium and the carbonate of potassium. The results obtained by using copper, platinum, and palladium in solutions of the carbonates and hydrates of sodium and potassium are given in Table III. :—

TABLE III.

	Sod. carb.	Pot. carb.	Sod. hyd.	Pot. hyd.
Copper . . .	853	883	867	896
Platinum . . .	831	827	772	756
Palladium . . .	740	758	777	645

8. Lead, silver, zinc, brass, and tin plate were tried in these same solutions, but they all (with the exception of brass in carbonate of potassium, which had a figure of merit 820) gave results far inferior to those recorded in Table III. The diacid carbonates were found to be far less energetic than the neutral carbonates. Increasing the specific gravity of the sod.-carb.

\* In this and all subsequent Tables the highest uniform results given by good experiments are recorded, as all the errors of observation, whether instrumental or functional, tended to diminish rather than to increase the figures of merit.

solution slightly raises the figure of merit, but not to an extent sufficient to counterbalance the disadvantage of a more rapid efflorescence on the plates. A very few drops of a solution of ammonia added to the sod.-carb. solution reduced its power by 75 per cent. A prolongation of the time of excitation caused an augmentation of the figure of merit, which, when that time is a variable, becomes a function of its reciprocal; and as copper in sod. carb. attained very nearly its maximum force after twenty seconds' excitation, whereas all other combinations required a much longer time, it is not thought necessary to burden this paper with Tables of the results obtained.

9. Although, as has been before stated (§ 4), by the mode of manipulation adopted we practically eliminate all external resistance (other than the resistance-coils) and their variations from consideration as regards the galvanometer circuit, yet the variation of the internal resistance of the Daniell cell, which affected the intensity of the exciting current, is of serious importance, and was, no doubt, one cause of the variations which appeared in the readings obtained on different days during the course of this investigation, although on the same day the results were very accordant. On this account the experiments were continued from day to day till a large number of results had been obtained; and from these it appears that the relative powers of the metals and solutions described in the Tables to this paper were always maintained.

10. There is another point to which attention was directed, which was this—that the secondary couples did not lose their power with the same rapidity, either during discharge or spontaneously; and as in the practical application of secondary batteries, as well as in their scientific aspect, the persistence of the current is of importance, experiments were undertaken to determine the rate of decadence of the powers of certain couples; and some of the results are recorded in Table IV. :—

TABLE IV.

Seconds'	Cu	Cu	Pt
rest.	sod. carb.	pot. hyd.	dil. sulph. ac.
20 . . .	12	5	30
40 . . .	17	8·5	43

In these experiments, after the Daniell cell had been cut off from the secondary couple, an interval of twenty or forty seconds was allowed to elapse before contact was made with the galvanometer, and in the above Table IV. we have the percentages of loss sustained by the secondary couples during those intervals. We see that copper in a solution of hydrate of potassium lost only 8·5 per cent. of its force during forty seconds, whereas platinum in dil. sulph. ac. lost in the same time 43 per cent.

11. To test the permanency of the secondary current during discharge, the resistance was reduced to 700 B.A. units, employing the 99 per cent. shunt. In the following Table V. we have the figures of merit of copper in sod. carb. and platinum in dil. sulph. ac. with 7000 units and 9 per cent. shunt, and 700 units and 99 per cent. shunt respectively :—

TABLE V.

Resistance . . . .	7000.	700.
Copper . . . .	853	821
Platinum . . . .	772	570

From this we see that, on diminishing the resistance of the secondary circuit by more than 6000 B.A. units, the platinum couple lost its force during the time (about three seconds) of the swing of the galvanometer-needles much more rapidly than the copper couple.

12. Copper\* plates were tried in a solution of the impure carbonate of sodium known in commerce as “soda;” and not only did they rise more quickly to their full power, but the maximum was far greater than that with the pure substance, giving a figure of merit 920—a value so high that, if it represented even the whole of the electromotive force generated in the secondary couple, we might be inclined to think that further experiments in this direction could lead to no better result; but a portion of that force was lost during the swing of the galvanometer-needles. It will be seen further on that, when working on a larger scale and with a more accurate method of measuring the electromotive force, we can obtain a figure of merit higher even than that just mentioned.

13. The force developed with the copper-“soda” couple for periods of excitation less than twenty seconds is shown in Table VI., where the power of the couple after twenty seconds is represented by 100 :—

TABLE VI.

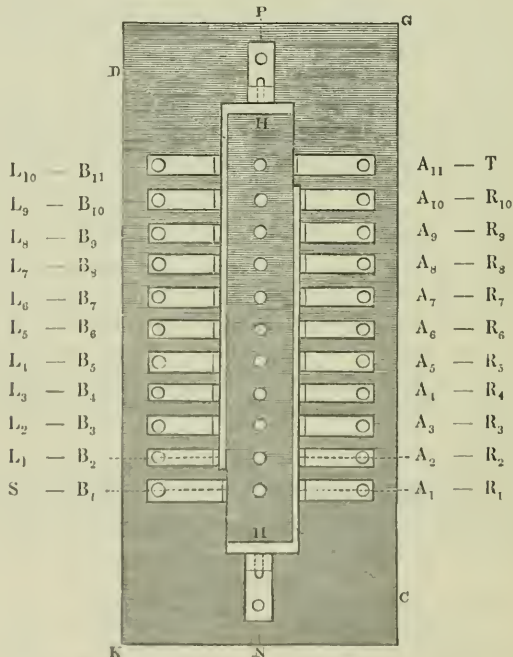
Period of excitation, in seconds.	Force of couple.
2 . . . . .	75
4 . . . . .	83
6 . . . . .	88
8 . . . . .	91
10 . . . . .	94
16 . . . . .	98
20 . . . . .	100

so that after ten seconds of excitation the force attained is within about 6 per cent. of the maximum.

\* In this, as in all other alkaline solutions, copper was found to be far superior to every other metal with which experiments were made.

14. The interposition of flannel between the plates of the experimental copper-"soda" couple having shown no tendency to diminish its force\*, a forty-cell secondary battery was constructed, each couple of which consisted of two plates of copper separated by flannel, each plate presenting an effective surface of 516 square centims., and was immersed in a solution of "soda," spec. grav. 1050. This battery was arranged in ten sets of four cells each, and each set was connected in series. A commutator was devised for the purpose of charging this battery as ten compound cells, and discharging it as forty cells in series. This instrument, shown in plan in fig. 2, consists of a

Fig. 2.

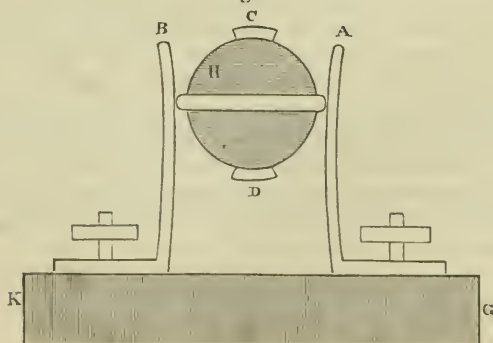


horizontal cylinder of ebonite, H H, attached to two vertical brass supports, P and N, and capable of being made to rotate about its axis through an angle of 90°. The supports P and N are fixed to an ebonite stand, G K, and are provided with screws for

\* Although I could not, after repeated excitations during many months, observe any deterioration in the condition of the flannel, yet no doubt an electrolytic action had taken place, as a purple dyeing-substance was produced, which gave a deep violet tint to the flannel and solution, the latter becoming fluorescent. We shall see further on that when a powerful exciting battery is employed, flannel is not suited to the purpose required.

connexion with the exciting battery. Eleven pairs of brass springs ( $A_1 B_1$ ), ( $A_2 B_2$ ), . . . . ., fixed to the ebonite stand and provided with screws for the necessary connexions, press against the cylinder in a direction transverse to its axis. Two narrow brass plates, C and D, are fixed to the cylinder on opposite sides, and are respectively in metallic communication with the supports N and P. In the position of the cylinder shown in this figure, these two plates are in a horizontal plane; and it will be seen that they do not extend the whole length of the cylinder, but that plate C is just clear of the spring  $A_{11}$ , and the plate D clear of the spring  $B_1$ , which springs are connected with the wires T and S respectively, through which the secondary discharge is to be taken. Between the letters H and H are seen eleven white circles, which represent the extremities of the same number of brass rods passing across the axis of the cylinder and extending slightly beyond its surface on each side; in the above-mentioned position of the cylinder the rods are vertical. The forty cells being arranged in ten sets of four each, let us call the right-hand terminal of each set  $R_1, R_2, \dots R_{10}$  in order, and the left-hand terminals,  $L_1, L_2, \dots L_{10}$ , in the same order. Then  $A_1$  and  $R_1$ ,  $A_2$  and  $R_2$ , . . . .  $A_{10}$  and  $R_{10}$ , and  $B_2$  and  $L_1$ ,  $B_3$  and  $L_2$ , . . . .  $B_{11}$  and  $L_{10}$  are respectively connected, as shown in the figure; and P and N are connected with the positive and negative poles respectively of the exciting battery, which consisted of five Grove cells. When the plates C and D are in a horizontal plane, the springs  $A_1, A_2, \dots A_{10}$  are pressing against the plate C, and therefore all the right-hand terminals,  $R_1, R_2, \dots R_{10}$ , are connected with the negative pole of the Grove battery; and similarly the left-hand terminals  $L_1 \dots L_{10}$  through the springs  $B_2 \dots B_{11}$  and the plate D, are connected with the positive pole, and the secondary battery is being charged. Let the cylinder be now turned through  $90^\circ$  into the position shown in fig. 3, which (having the same lettering as fig. 2) re-

Fig. 3.





presents a vertical section of the instrument through a pair of springs across the axis of the cylinder. The plates C and D are thrown out of contact with the springs, which, through the now horizontal brass rods across the cylinder, become connected metallically in pairs, as shown in the figure 3; *i. e.* A<sub>1</sub> to B<sub>1</sub>, A<sub>2</sub>to B<sub>2</sub>, . . . A<sub>11</sub>to B<sub>11</sub>, so that R<sub>1</sub> is connected with S, R<sub>2</sub>to L<sub>1</sub>, R<sub>3</sub>to L<sub>2</sub>, . . . Tto L<sub>10</sub>, the whole ten sets being thus joined together in zigzag fashion; the forty cells will then be connected in series and discharged through the conductors S and T. Complicated (and, it is feared, too much so) as this description may be, the instrument and its action are in fact very simple; and as this apparatus "functions" remarkably well, and may be used for other and analogous purposes, the account has been made somewhat elaborate.

15. The current from this battery had for a secondary current considerable duration; after one excitation, repeated deflagrations could be obtained with tin- or platinum-foil; with the latter of the thickness of .1 millim. a hole was pierced 4 milims. in diameter, the edges of which were fused. Its electromotive force was measured by a Thomson's portable electrometer, and the results are given in Table VII.

TABLE VII.

	Potential.
Five Grove cells . . . . .	3
Forty secondary cells . . . . .	19
Forty secondary and five Grove cells .	22

(In order to throw the Grove battery into the same circuit and in the same direction as the secondary battery, a very simple and obvious addition was made to the connexions.) These results were repeatedly obtained without any variation, and are in complete accord. The electromotive force, therefore, of the forty cells was equal to that of 31.6 Grove cells.

16. After some months' inaction this battery had seriously declined in power; and on examination it was found that the flannel was almost entirely destroyed. The couples were therefore reconstructed, and on a smaller scale. Each of the copper plates in each pair now made exposed an effective surface of 108 square centims., and was separated from the other by pieces of ebonite at a distance of rather less than 2 millims. On charging forty of these couples by five Grove cells, and connecting the terminals with the electrometer, the following results were obtained:—

TABLE VIII.

	Potential.	Equivalent number of Grove cells.
Five Grove cells . . . .	3	5
Forty secondary cells . .	23	38·3
Do. with five Grove cells .	26	43·3

Now, since the electromotive force of the forty secondary cells in series is represented by 23, that of five cells will be 2·875; and as the force of the five Grove cells is expressed by 3, we see that the force of each set of five secondary couples is 95·83, or nearly 96 per cent. of that of the exciting battery, a result which can scarcely be greatly surpassed. An experiment was made to test the usefulness of this form of battery for telegraphic purposes. The discharge was sent through 19,000 B.A. units, or more than twice the resistance of the Atlantic cable, and was received at an indicator such as is employed only to test the continuity of telegraph wires, and which is a comparatively coarse instrument. The deflection of the needle when contact was first made was  $35^{\circ}$ , and at the end of one minute, the contact having been continued during that time,  $20^{\circ}$ ; whereas the five Grove cells could of themselves produce but a very feeble motion of the needle, of  $2^{\circ}$  or  $3^{\circ}$  only. An excitation of five seconds' duration appeared to completely restore to the secondary battery its original power.

17. There is one disadvantage attending the use of the carbonate-of-sodium solution in secondary batteries, viz. the efflorescence on the plates and cells. Many different substances, such as shellac in alcohol, oil, lard, and black enamel varnish, have been tried, but sooner or later the crystals of the carbonate creep either under or over the varnish. Perhaps hot paraffine poured into the top of the cells when the battery is first made, and, after cooling, a hole bored to allow of the escape of any gas generated, might answer the purpose of checking the efflorescence.

Hadham House, Upper Clapton,  
December 13, 1869.

IX. *Reply to some Remarks of Father Secchi on the recent Solar Discoveries.* By J. NORMAN LOCKYER, F.R.S.\*

THAT portion of a recent note (communicated to the Royal Society and printed in the *Comptes Rendus* of the 12th of July) which had reference to Father Secchi's observations, had

\* Communicated by the Author.

for its object, as I clearly stated, the settlement of certain points where our observations were not in agreement, in order that other workers might employ the new method of observation under the best conditions.

I gather from Father Secchi's communication—

I. That he still holds to a "*couche donnant un spectre continu, couche qu'il considère comme la base de l'atmosphère solaire, et dans laquelle, il pense que s'effectue le renversement selon la théorie de Kirchhoff.*"

II. That he has doubts as to the importance which I attribute to the widening of the F line at its base, on which Dr. Frankland and myself have founded our estimate of the pressure of the chromosphere.

III. That he also has doubts as to the changes of wave-length in the hydrogen-lines, which I have asserted to be continually visible both on and off the sun.

IV. That he holds still that the line F is due to the absorption of some other substance besides hydrogen.

With regard to all these points I am quite content to leave the verdict to the future. I may, however, remark with regard to the first point, that although I see nothing like a continuous spectrum, I do see traces of reduced absorption in the exterior layer of the photosphere; and Dr. Frankland and myself have proved why the absorption increases as the lower layers are brought into action, as in a spot. Father Secchi has written\*, "*Ayant examiné comparativement le spectre du noyau des taches et celui du bord du disque du côté intérieure, je suis arrivé à la conclusion que ces deux spectres se ressemblent considérablement. L'élargissement des raies constaté dans le noyau se reproduit près du bord.*" I do not find this; but it appears to me that this is a contradiction in terms to his assertions about the continuous spectrum of these regions.

With regard to the second, in which the action of pressure is in question, I again quote †:—"J'ai encore porté mon attention sur la largeur des raies brillantes de la chromosphère et j'ai constaté qu'en général les raies principales sont toutes trois plus large à la base qu'au sommet, *ce qui prouve l'influence de la pression exercée par les couches supérieures.*" This strikes me as another contradiction in terms, and Father Secchi must take his choice between these assertions. But I must do him the justice to admit that he early thought something could be made out by means of pressure experiments, for he recommended that the spectrum of hydrogen *sous une très forte pression* should be observed, while Dr. Frankland and myself were working successfully *near the vacuum-point*.

\* *Comptes Rendus*, 2<sup>e</sup> sem. 1869, p. 41.

† *Ibid.*

Passing from these major points, then, which, as I said before, I am content to leave to the future, there are several minor ones on which I beg the Academy will allow me to say a few words.

Father Secchi says, "Lorsque M. Lockyer affirme m'avoir précédé dans les observations des injections de magnésium, on ne peut pas dire jusqu'à quel point cela est exact." So far as the mere question of priority goes, I should not think this remark worth answering; but as Father Secchi seems to cast doubt on my veracity, I beg to refer him to a paper communicated to the Royal Society on March 4, 1869, the first observations, as there stated, having been made on February 21 and 28. With regard to his assertion, "J'ai clairement vu et démontré que seulement une ligne du magnésium est renversée et que l'autre ligne brillante occupe l'espace intermédiaire des deux plus voisines," I feel myself justified in denying the accuracy of the observation, and I leave this, like the other questions, to be decided by the future. I should not venture to do this if a long series of experiments at the Royal College of Chemistry had not been absolutely in accord with my telescopic observations which have already been described in the *Comptes Rendus*.

M. Secchi objects to my mixing up theory and observations. I plead guilty. I confess a remark made some time ago by M. Faye is always present with me when I am observing. The remark is, "A good theory is as necessary as a good telescope." Without a working hypothesis, I should certainly have cross-questioned the sun much less than I have done; and it should be a truism that in a research, such as the one we are now conducting, it will not do to observe blindly or haphazard. For instance, I began on the generally received theory that the absorption took place outside the photosphere, which is evidently Father Secchi's present idea, as shown by the extract from one of his latest communications I have already given, but by testing the theory in every way, I found it untenable; and I venture to think that if Father Secchi had done the same, there would have been less contradiction in his statements, and he also would have found such a theory untenable. But I confess that the remark that it is too soon to theorize, coming from Father Secchi, somewhat surprises me; for I find very many theories referred to in his earliest papers on the subject.

In my former paper I stated, "si la chromosphère était suspendue à une certaine distance de la photosphère, nous ne pourrions trouver un élargissement dû à la pression." On this Father Secchi remarks that he does not see the justice of my conclusion. This may arise from the fact that he does not agree that the H line widens by pressure; that is, if he elects



to hold to that statement instead of the contrary one; but my remark simply meant that if the chromosphere were thin instead of thick, that is, if it did not extend to the photosphere, there would be less room for pressure to make itself evident. He then adds, “cette structure des masses suspendues dans une atmosphère . . . . résulte . . . . des observations des éclipses.” Here I would ask where such observations of the chromosphere (for the prominences are not in question) are recorded, and how such observations, if they exist, could help us in such a question.

I next read, “il est impossible d’admettre que ces images ou ces colonnes inclinées puissent rester suspendues, sans un milieu qui les supporte et qui soit différent d’elles-mêmes.” The reply to this is, that if Father Secchi or any one will prove the existence of this medium, I will willingly admit it; but I have to bear in mind:—

I. That although Dr. Frankland and myself have shown that the temperature is high enough in the prominence-region to render other substances incandescent, if they were there, we have no spectroscopic evidence of their existence.

II. That the tenuity of the prominences is excessive.

III. That the prominences are not static, but are rapidly driven beyond the upper level of the chromosphere, and as rapidly vanish.

IV. That there is no cooler hydrogen above the chromosphere, or we should get the F line alone.

V. That hydrogen is the lightest gas.

Father Secchi claims my observations of the injections of sodium, magnesium, &c. INTO the chromosphere as supporting the existence of his continuous-spectrum-giving stratum BELOW the chromosphere. I ask how is this possible? Moreover my observations show, I think, that the vapours of sodium, magnesium, &c. lie immediately below the photosphere; and how can they there give a continuous spectrum if they do not above and do not below?

In my former paper I said that, by using three prisms, the spot-phenomena were possibly as complicated as Father Secchi has described them, but that with my greater dispersive powers this complication vanishes to a great extent. On this Father Secchi states that I “cherche à mettre mes résultats en opposition avec les siens.” Remarking that my object was the very opposite, I again quote from one of Father’s Secchi’s latest papers\*, a later one than that in which he states that the spectrum of a spot is similar to the spectrum of the limb:—“Il n’y a pas production des raies fondamentales nouvelles, mais seulement un renforcement considérable des raies solaires connues comme déjà exis-

\* *Comptes Rendus*, 1869, 2<sup>e</sup> sem. p. 166.



tantes." Here again, then, I must leave Father Secchi to settle the matter with himself. With regard especially to the bright lines at times visible in spots, which Father Secchi considers due to the radiation of the interior gaseous nucleus of the sun, I can only state that I have seen in the spots no bright lines which are not at times visible in the ordinary solar spectrum; it is true that in the spots they are better seen.

In conclusion, I may state:—that the method of observation I employ, of which I regret to find Father Secchi thinks so little, consists in observing the actual images of the sun cast by a reflector of 625 inches aperture and 100 inches focal length, or an image enlarged to 6 inches in diameter, according to the state of the air; that my spectroscope is furnished with a train of seven prisms of the densest flint glass, giving me a refracting-angle of over  $300^{\circ}$ ; that this dispersion is increased by another dense flint-glass prism of  $60^{\circ}$ , and a direct-vision prism of equal power; and, finally, that I am not satisfied with this dispersion, which is more than double that employed by Father Secchi, and in a few days I hope to double my present power.

## X. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from vol. xxxviii. p. 465.]

June 17, 1869.—Lient.-General Sabine, President, in the Chair.

THE following communications were read:—

“On Jargonium, a new Elementary Substance associated with Zirconium.” By H. C. Sorby, F.R.S. &c.

At the Soirée of the President of the Royal Society on March 6th, I exhibited various spectra, differing so much from those characteristic of any known substance, that I considered myself warranted in concluding that they were evidence of a new element. Since this may be studied to the greatest advantage in the jargons of Ceylon, it appeared to me that, like as the name zirconium has been adopted for the principal constituent of zircons, so that of jargonium would be very suitable for this constituent of jargons.

At the above-named Soirée I gave away a printed account of the objects I exhibited, and in this I said that the earth jargonite “is distinguished from zirconia and all other known substances by the following very remarkable properties. The natural silicate is almost, if not quite colourless; and yet it gives a spectrum which shows above a dozen narrow black lines, much more distinct than even those characteristic of salts of didymium. When melted with borax, it gives a glassy bead, clear and colourless both hot and cold, and no trace of absorption-bands can be seen in the spectrum; but if the borax

*Phil. Mag.* S. 4. Vol. 39. No. 258. Jan. 1870.

F

bead be saturated at a high temperature, and flamed, so that it may be filled with crystals of borate of jargonia, the spectrum shows four distinct absorption-bands, unlike those due to any other known substance."

I have since applied myself almost exclusively to this subject, hoping to be able to communicate to the Royal Society a full account before the close of this session; but so much still remains to be done, that it is now impossible to give more than a brief outline of some of the more important facts. The delay has not been occasioned by any difficulty in proving it to be a new substance, but because its properties are so unique and have so much interest in connexion with physics that it appeared desirable to carefully examine all other known elements, in order to ascertain whether any exhibit analogous phenomena.

That jargonium is quite distinct from zirconium is proved not only by the spectra, but also by other facts. Both I and Mr. David Forbes have succeeded, by entirely different processes, in separating from jargon zirconia apparently quite free from jargonia, and jargonia nearly, if not quite, free from zirconia; and even if the separation be not perfect, it is, at all events, more than sufficient to prove that they are distinct. They are certainly closely allied, and are deposited from borax blowpipe beads in microscopical crystals of the same general forms, quite unlike those characteristic of other known earths; but beyond this the difference is as great as that between any other two closely related elements. Judging from Mr. D. Forbes's analysis, kindly made at my request, and from a comparison of the spectra, the amount of jargonia in different jargons varies up to about 10 per cent. Its entire or comparative absence from the zircons of Miask, Fredericksvärn, and various other localities, appears to explain some of the facts which led Svanberg\* to conclude that zircons contain more than one earth. He was so far correct, but failed to establish the existence of any substance with special chemical or physical properties; and if, as is probable, the Norwegian zircons, which, according to his views, contain such a notable quantity of this supposed new earth as to have led him to give it the name *norita*, were from Fredericksvärn, and if the Siberian were from Miask, his *norium* cannot be looked upon as equivalent to my jargonium, which is almost or quite absent from those zircons.

The most remarkable peculiarity of jargonium is that its compounds may exist in no less than three different crystalline states, giving spectra which differ from one another as much as those of any three totally different elements which give the most striking and characteristic spectra. Several substances can be obtained in two physical states, giving different spectra; but usually only one of them is crystalline, the other is the vitreous or colloid condition. Crystalline minerals, coloured by oxide of chromium, do indeed show two types of spectra, but I am not aware that they

\* Pogg. Ann. 1845, vol. lxx. p. 317.

ever both occur in the same mineral. In the case of jargonium, however, the three types of spectra are all met with in *crystalline* modifications of apparently the *same* compound.

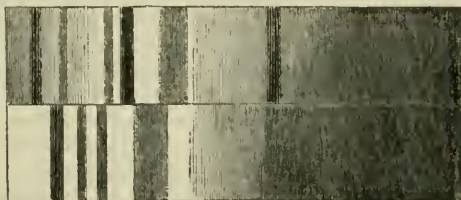
The most characteristic test for jargonia is the spectrum of the borax blowpipe beads, seen with the spectrum-microscope, which enables us to detect it in zircons containing less than one per cent. As much of the earth or natural silicate as will completely dissolve should be melted in circular loops of platinum wire, about  $\frac{1}{8}$  of an inch in diameter, with a mixture of borax and boric acid, and a very strong heat kept up till crystals begin to be deposited, owing to loss of the solvent by volatilization. On removing the beads from the flame they remain clear, and show a few acicular crystals, but give no absorption-bands in the spectrum. On reheating to a temperature just below very dull redness, they turn white, and so very opaque that no ordinary light will pass through them. When, however, a small and very bright image of the sun is formed in their centre, by means of an almost hemispherical condensing lens of  $\frac{1}{2}$  inch diameter, and a cap placed over the object-glass, with a round hole less than the beads nearly in the focus, so as to prevent the passage of extraneous light, they are seen to be illuminated by transmitted light of about the same brilliancy as that of a bright cloud, so as to give an excellent spectrum, without being at all dazzling. In the case of beads containing jargonium, the spectrum differs completely according to the temperature at which the included crystals have been deposited. As already mentioned, a clear glassy bead gives no absorption-bands; and when the crystals are deposited at as low a temperature as possible, much below dull redness, and only just high enough to soften the borax, there may be scarcely any trace of bands; but if a clear bead be quickly raised to a temperature very little below dull redness, it suddenly becomes opaque, and shows a spectrum with a number of narrow black absorption-bands (fig. 1). The most distinct is in the green, then one in the red, and one in the blue; and there are three fainter—one in the orange, and two in the green. On raising the temperature to bright redness all these bands vanish, and four others appear, none of which coincides with the former (fig. 2). Three are situated in the red and orange, and one in

Red end.

Blue end.

Fig. 1.

Fig. 2.



the green, so as to give a spectrum of very different general character. In this state the bead is a pale straw-colour, and not, as before, almost white. In the case of nearly pure jargonium, the

bead should not be more than  $\frac{1}{25}$  of an inch thick, or else it would be too opaque. Pure zirconia treated in the same manner gives no bands whatever in any condition; the bead is quite white, and sufficiently transparent when two or three times as thick as just named.

It might be thought that the three different spectra thus briefly described were due to different compounds, if it were not that there is a similar series in the case of the natural crystalline silicate. Some of the jargons of Ceylon have a specific gravity very little inferior to that of pure zircons (4.70), and contain very little jargonia; but those of low gravity (4.20 or thereabouts) contain perhaps nearly 10 per cent., in a form which gives scarcely any trace of absorption-bands. On keeping such a specimen at a bright red heat for some time, the specific gravity increases from about 4.20 to 4.60. Judging from the imperfect data now known, this indicates that the volume of the silicate of jargonia is reduced to about one-half; the hardness becomes somewhat greater, and, when examined with the spectrum-microscope, the spectrum is found to be entirely changed. Instead of a mere trace of bands, a spectrum is seen with thirteen narrow black lines and a broader band, more remarkable than that of any clear transparent substance with which I am acquainted. No such changes occur in the case of zircons free from jargonia, like those from Miask, Siberia; there is no increase in the specific gravity, and no absorption-bands are developed; and, as a general rule, the increase varies simply and directly as the amount of jargonia which passes from one state into the other. Zircons in their natural condition from various localities contain a very variable absolute and relative amount of these two modifications of jargonia, and there seems good reason to believe that this difference in physical state may materially assist us in determining the temperature at which certain rocks have been formed. I have also met with one example of the third form of spectrum. A brown-red zircon from Ceylon was so dark in one part as to be quite opaque, and therefore I do not know what the original spectrum might have been. On heating it to redness, the whole became a clear pale green; and, without examination with the spectroscope, no one would have suspected any difference between the different portions. That which was originally a pale brown-red then showed the same spectrum as that usually developed by heat, whilst that which was originally very dark showed an entirely different spectrum, corresponding exactly with that of the borate deposited in blowpipe beads at a medium temperature. It also corresponds in general character, but not in detail, with that of the blue spinels from Ceylon, which must, I think, contain a small quantity of jargonia. That part of the zircon which gave this spectrum appears to have had the same remarkably low specific gravity of about 4.0 both before and after ignition, as though the volume of the silicate of jargonia in this state were even greater than in that which gives no bands. All these spectra due to jargonium are of a very marked character, and quite unlike those due to any other element in similar conditions.



The alteration produced in jargons by heat is, to some slight extent, analogous to what occurs on heating carbonate of lime in the state of arragonite; but, instead of changing into an opaque mass of minute crystals of another form of the carbonate (calcite, which has a less specific gravity, is less hard, and does not give a different spectrum), they are still as simple and transparent crystals as at first, the specific gravity and hardness are increased, and the spectrum is entirely changed. Iodide of mercury is an excellent illustration of an alteration in the spectrum, due to a change in crystalline form produced by heat; but still the facts differ most materially from those described, and there are only two modifications—the yellow and the scarlet. The existence of three crystalline modifications is similar to what occurs in titanite acid. Anatase, Brookite, and rutile have distinct crystalline forms; but they do not differ much in specific gravity, and their spectra present no characteristic differences. On the whole, the different states of carbon (charcoal, graphite, and diamond) are perhaps the best illustration of the existence of three different conditions in the same substance, since they differ materially in specific gravity and optical characters, one being black, the other having a metallic lustre, and the third being transparent and colourless; but these are variations of the element itself, and not, as in the case of jargonium, modifications of its compounds. So far as I am aware, there is indeed no substance which shows strictly comparable facts.

There cannot, then, I think, be any doubt whatever that jargonium is not only a new elementary substance, but is also one likely to throw much light on several important physical questions. By the time that the Society resumes its meetings, I trust that I shall be able to send a complete account of the whole of my investigations, including such facts connected with other substances as may serve to illustrate the very peculiar properties of this hitherto unrecognized element.

#### POSTSCRIPT.

I here subjoin a brief account of the methods employed by Mr. David Forbes\* and myself in separating zirconia and jargonia from one another. He separated apparently pure zirconia by means of strong hydrochloric acid, which dissolved the chloride of jargonium, but left chloride of zirconium undissolved; and obtained the approximately pure jargonia by adding to the solution excess of ammonia, and then considerable excess of tartaric acid, which left most of the tartrate of jargonia insoluble, but dissolved what may turn out to be a mixture of zirconia and jargonia with a third substance, not yet sufficiently studied—perhaps Svanberg's *noria*. My own analysis was only qualitative. I fused powdered jargon with several times its weight of borax, which gave a perfectly clear glass, completely soluble in dilute hydrochloric acid. After separating the

\* *Chemical News*, June 11, 1869, vol. xix. p. 277.



silica in the usual manner, a slight excess of ammonia was added to the hydrochloric-acid solution of the earths, and then some oxalic and hydrochloric acids, which left undissolved apparently pure zirconia that had passed into an imperfectly soluble state. To the solution so much ammonia was added as to give a very copious precipitate, but yet to leave the solution with a very decided acid reaction. After removing the precipitate, which was chiefly oxalate of zirconia almost or quite free from jargonite, excess of ammonia was added to the solution, and the washed precipitate digested in dilute hydrochloric acid, to remove peroxide of iron. The insoluble portion must have been approximately pure oxalate of jargonite; for it gave the characteristic spectra described in remarkable perfection. Though this method succeeded far better than I anticipated, I do not yet understand the exact conditions requisite to ensure success, and have been prevented by absence from home from making further experiments.

“Solar Radiation.” By J. Park Harrison, M.A.

In a communication which the author had the honour of making to the Royal Society in 1867\*, it was shown, from observations of the black-bulb thermometer and Herschel's actinometer, that maximum effects of solar radiation occur at Greenwich, on the average, some weeks after the summer solstice, and about two hours after midday, when the atmosphere would appear to be charged with a considerable amount of vapour.

These results accord with the fact that the highest readings of the solar thermometer are met with in India in districts of great relative humidity†, the explanation of the phenomenon being, as the author ventured to suggest in the paper above alluded to, that an increase of insolation is produced by radiation from cloud and visible vapour.

During the two years which have elapsed since the spring of 1867, whenever the state of the sky and other circumstances permitted, special observations have been made for the purpose of ascertaining with greater certainty the nature of the relation between insolation and humidity.

Before proceeding to state results, it will afford additional proof that a connexion between the phenomena really exists, if a passage in the appendix to a work by the late Principal of St. Andrews, until very recently overlooked, is quoted in support of the fact. Mr. Forbes, writing some years ago, employs much the same words that were used in the paper above referred to:—“Cloudy weather, if the sun be not itself greatly obscured, apparently increases the effect of solar radiation”‡.

The action, however, does not appear to be confined to days on which there is *visible* cloud; for even on cloudless days (so called) very high readings of solar radiation seem to be due to the presence

\* Proc. Roy. Soc., Feb. 1867.

† Proc. Roy. Soc., March 1865.

‡ *Travels through the Alps of Savoy*, App. III. p. 417.

of opalescent vapour, which can be easily detected if the hand or some other screen is held for a few minutes before the sun.

Thus, on May 2, 1868, at 1<sup>h</sup> 30<sup>m</sup>, solar radiation appearing to be relatively intense, on raising a screen white glare was observed around the sun, and the tint of the sky, which had previously appeared a fair blue, was found, more especially in the south, to be very pale.

But the most interesting result of this series of observations is the discovery that an apparent increase of solar radiation occurs as the sun enters a white cloud of sufficient tenuity to allow free passage for its rays.

In October 1867, at 2<sup>h</sup>, whilst attentively watching a solar thermometer, a sudden rise was observed to take place, upon which, the sun being immediately screened, it was found that it had entered the bright border of a cumulus.

On May 11, 1868, at 22<sup>h</sup> 40<sup>m</sup>, as a very light cloud approached the sun, which was shining in blue sky, the mercury rose 4°, and in 30 seconds 3° more as it entered the white cloud.

On the same day, at 23<sup>h</sup>, the reading of the solar thermometer was 101°F. when the sun was in the midst of cirri, but it fell in 3 minutes 9° when well free again; then rose 6° as light cloud again crossed it. The air was perfectly still.

On May 15, 1868, the highest reading of the solar thermometer for the day occurred at 2<sup>h</sup> 17<sup>m</sup>, just as the sun entered the skirts of a cloud.

On July 21, 1868, at 2<sup>h</sup>, the maximum of the day (128° F.) was reached when the sun was shining in a patch of pale sky surrounded with white cumuli, some of which were within one or two diameters of its disk.

To mention one more example amongst numerous others which might be cited: on Aug. 3, 1868, at 0<sup>h</sup> 40<sup>m</sup>, under an apparently clear sky, the solar thermometer registering 112°, and the temperature of shade 82°, in two minutes insolation increased to 125°, whilst the temperature of shade rose 0·3 only; on examining the sky in the neighbourhood of the sun, white cirri were detected crossing its disk.

Light cloud and opalescent vapour having been thus found, when in the direction of the sun, to intensify the effects of solar radiation, a series of experiments was commenced with circular screens of various sizes, to discover, if possible, *the distance* to which the effects of bright glare and light vapoury cloud extended round the sun.

The observations were made when the sun's altitude was between 30 and 50 degrees. All the screens were placed at a uniform distance of six inches from the bulb of a solar thermometer  $\frac{1}{4}$  in. in diameter, coated with China ink, and laid on a small piece of dark oak about two inches by ten inches on grass\*. The bulb of the

\* Similar results were obtained when the solar thermometer was laid upon short grass, in the afternoon, when the dew was off the ground.

With the instrument freely suspended 6 in. above the grass, the readings showed a proportionate fall.

thermometer was not covered with an exhausted globe. The mean results of the experiments were as follows :—

1. A screen  $\frac{1}{2}$  in. in diameter reduced the difference of the readings of the black-bulb thermometer and a thermometer in the shade, four yards distant, by one-third.

2. A screen  $2\frac{1}{2}$  ins. in diameter reduced the difference by two-thirds.

On reversing the experiment, converse results were obtained, *e. g.*

The rays of the sun, after passing through a circular aperture  $2\frac{1}{2}$  ins. in diameter in a 12-in. screen, were made to fall on the bulb of the solar thermometer, when the readings were found to equal in value those obtained when the instrument was entirely exposed\*.

And no difference was noticed when the black-bulb thermometer was screened from the rest of the sky by a double cover of mill-board placed tent-wise over it.

Results of an equally negative kind were obtained in the case of other experiments which were made with the object of detecting heat in the light reflected from sky and cloud not in the direction of the sun.

A black-bulb thermometer, after having been placed for some time in a dark room, was then exposed to the sky, near a large French window, facing S.E., the glass of which was clear, and had been carefully cleaned, without any rise being perceptible. The sun, at an altitude of about  $40^\circ$ , was shining brightly on white vapour and light cirro-cumuli†.

Thermometers were also placed in the open air on the north side of the house, on a still day, exposed to half the sky when covered with bright white clouds; but the mercury stood at the same height as in a dark passage on the same side of the building‡.

The same apparent absence of any direct heating-power in the light reflected from the sky generally was shown in this as in the previous series of experiments when the solar thermometer was screened excepting in the direction of the sun.

As respects the momentary increase of insolation which occurs in connexion with bright vapour in the neighbourhood of the sun, further experiment is required for the purpose of ascertaining whether it is due to radiation or to reflection.

NOTE.—An opportunity occurred on the 7th of June of repeating the experiments with screens at altitudes of the sun exceeding  $50^\circ$ . The following results were obtained :—

\* In the above experiments, it is evident that the whole of the results were not due to direct radiation or reflection. Account must be taken of the greater or less distance of the heated surface of the ground, and of the hot air in contact with it, from the bulb of the solar thermometer.

† Experiments were also tried with a 7-inch lens, without result.

‡ The thermometer exposed to the sky would probably have stood *lower* than the one in the house if the sky had been perfectly clear.

h m					
At 0	0.	B. B. 110.	Temp. of shade 73.	{	Sky cloudless, but with a good deal of white vapour, more especially about the sun.
		(Exposed to the sun and sky.)			
	0 4.	B. B. 90.	Temp. of shade 73.		" "
		(Shaded from sun by a 2-in. screen.)			
	0 30.	B. B. 104.	Temp. of shade 73.		Light air.
		(Exposed to sun and sky.)			
	0 35.	B. B. 94.	Temp. of shade 73.		Light air.
		(Shaded from sun by a $\frac{1}{2}$ -in. screen.)			
	1 0.	B. B. 108.	Temp. of shade 74.		Quite calm.
		(Exposed to sun and sky.)			
	1 5.	B. B. 109.	Temp. of shade 74.		Quite calm.
		(Exposed to sun through a 2-in. circular aperture in a 12-in. screen.)			
	1 15.	B. B. 108.	Temp. of shade 74.		Quite calm.
		(Exposed to sun and sky.)			
	1 18.	B. B. 106.	Temp. of shade 74.		Quite calm.
		(Exposed to sun through a 2-in. circular aperture in a 12-in. screen.)			
	1 20.	B. B. 106.	Temp. of shade 74.		Quite calm.
		(Exposed to sun but screened from sky.)			

## XI. Intelligence and Miscellaneous Articles.

ON THE LAWS OF INDUCTION. BY MM. JAMIN AND ROGER.

THE experiments we are about to describe were arranged like those which are made on induction. A current interrupted at regular intervals by a mercury contact-breaker traverses an inducing coil the resistance of which is  $b$ ; in this is placed the induced coil, the resistance of which is  $\beta$ ; in the centre is a bundle of annealed iron wires. The two ends of the inducing coil are connected with the two armatures of a Fizeau's condenser; lastly, both the induced and the inducing coil may be lengthened by scales of varying resistance, which we shall designate by  $\theta$  and  $t$ , and which may be increased from zero to infinity.

The peculiarity of our apparatus is that the coils, the soft iron, the condensers, and the resistances  $\theta$  and  $t$  are immersed in an insulating liquid (oil of turpentine or benzole) in separate vessels hermetically closed and terminated by divided thermometric tubes. By means of a levelling tube with a stopcock the liquid in these tubes may always be brought to a fixed height; each apparatus constitutes therefore a thermorheometer. As its electrical motion creates a quantity of heat proportional to its *vis viva*, the liquid receives this heat, and the expansion measures it. We shall omit any details as to these experiments, and restrict ourselves to giving a *résumé* of the facts.

*Induced current.*—The induced coil is connected with the scale of resistance  $\theta$ , which varies from zero to infinity. The quantities of heat,  $C, C_1$ , produced in the unit of time in this resistance and in this coil are measured. We may always write

$$C = \frac{\theta I'^2}{\alpha}, \quad C' = \frac{\beta I_1'^2}{\alpha},$$

$\alpha$  being the total duration of the passages of the inducing current



during one minute,  $1-\alpha$  that of the breaks. Calculating  $I'^2$  and  $I_1'^2$ , we find them to be equal, and that they satisfy the relation

$$I' = \frac{M\alpha}{N + \beta + \theta}.$$

Now, if the wire of the induced coil  $\beta$  and that of the resistance were extended in a straight line so as to avoid induction, and if the current of a battery were passed the electromotive force and resistance of which were  $M$  and  $N$ , we should have a current of the real intensity  $\frac{M}{N + \beta + \theta}$ ; and if it were opened at regular intervals in such a manner that the duration of the passage were reduced from 1 to  $\alpha$ , we should have a mean intensity

$$I' = \frac{M\alpha}{N + \beta + \theta}.$$

Further, the amounts of heat developed in the resistance  $\theta$  and in the coil  $\beta$  would be, according to Joule's law,

$$C = \frac{\theta I'^2}{\alpha}, \quad C' = \frac{\beta I'^2}{\alpha}.$$

The same would be the case if the currents were alternately in opposite directions. Inverting this reasoning, we are led to identify this hypothetical case with that of induction, and to formulate the law of the induced current as follows.

1. The successive induced currents, although alternately in opposite directions and of varying intensity, have the same mean intensity  $I'$  as if they proceeded from a battery with constant current;  $I'$  is expressed by Ohm's law,

$$I' = \frac{M\alpha}{N + \beta + \theta}.$$

2. The quantities of heat  $C$  and  $C'$  which these currents develop either in an external circuit  $\theta$  or in the coil  $\beta$ , are the same as those which would be created in these resistances by this hypothetical battery.  $C$  and  $C'$  are expressed by Joule's law,

$$C = \frac{\theta I'^2}{\alpha}, \quad C' = \frac{\beta I'^2}{\alpha}.$$

3. The electromotive force  $M$  of these currents is far greater than that of the inducing battery which sets the whole in action; and the resistance  $N$  is itself far more considerable than the resistance  $\beta$  of the coil.

4.  $M$  and  $N$  being both very great, the mean intensity  $I'$  is very small; whence it is that the inducing current produces but small chemical actions, although traversing great resistances and giving shocks. If  $\theta$  is infinite, the current is zero, there is no heat in the wires; but their extremities assume alternately contrary tensions, which are very great, since the electromotive force  $M$  is considerable. It is the case of Ruhmkorff's inductorium.



The experiment may be varied by placing the two coils in the same vessel, and measuring, on the one hand, the sum of the heats created by the inducing and induced currents, and, on the other, that which is produced in the external resistance  $\theta$ . This is given by the formula

$$C = \frac{\theta I'^2}{\alpha}.$$

The values of  $I'$  are deduced from this; and it is found, as we shall see in the Table, that they satisfy the law

$$I' = \frac{M\alpha}{N + \beta + \theta}.$$

$$M = 87.23, \quad N + \beta = 13.00, \quad \alpha = 0.47.$$

<i>t.</i>	<i>I'</i> .		<i>I.</i>		Sum.				
	Ob-served.	Calcu-lated.	Ob-served.	Calcu-lated.	$\frac{\beta I'^2}{\alpha}$ .	$\frac{\delta I^2}{\alpha}$ .	$\frac{\beta I'^2}{\alpha} + \frac{\delta I^2}{\alpha}$ .	<i>w.</i>	Differ-ence.
1.06	3.147	2.916	4.680	4.772	23.62	103.00	126.62	130.75	+ 4.13
2.12	2.958	2.711	4.365	4.475	20.87	89.56	110.43	123.06	+ 12.63
4.26	2.447	2.378	4.267	4.254	14.31	85.62	99.93	99.44	— 0.49
6.36	2.101	2.117	3.899	3.945	10.50	71.50	82.00	83.69	+ 1.69
8.48	1.833	1.908	3.865	3.918	8.00	66.06	74.06	80.18	+ 6.12
12.72	1.509	1.594	3.748	3.767	5.44	58.81	64.25	72.18	+ 7.93
22.82	1.075	1.144	3.537	3.605	2.75	58.06	60.81	63.87	+ 3.06
32.72	0.869	0.896	3.513	3.575	1.81	56.12	57.93	60.56	+ 2.63
51.72	0.648	0.633	3.455	3.510	1.00	59.62	60.62	58.94	— 1.68
81.22	0.459	0.435	3.560	3.586	0.50	60.37	60.87	61.00	+ 0.13

*Condenser.*—When the experiments are made without using the condenser, sparks are seen to pass in the contact-breaker between the platinum point and the mercury. These are due to the extra current produced at each breaking of the circuit; its variations are indicated by the lustre and noise of these sparks. Now they are feeble if the resistance  $\theta$  added to the induced coil is small; they increase with  $\theta$  to a maximum when  $\theta = \infty$  (that is, when the coil is open). Hence the extra current becomes small or increases with  $\theta$ .

These sparks have two inconveniences: first, they divide the mercury, make the alcohol turbid, and increase its conducting-power; then also they expend the heat of the pile. The condenser remedies these defects, annuls the sparks, prevents the turbidity of the liquid and the increase of its conductivity; it also annuls the heat, which is transported from the cup, where it is not measurable, into the interior of the condenser, where it can be measured.

The following is the working of the instrument. At the moment of rupture the extra current has two paths—one by the contact-breaker and the battery, the other by the condenser; this latter presents least resistance; this current collects there in the form of two charges of high tension, positive and negative, which condense on the armatures as long as it lasts, and recombine through the coil

when it has ceased, leaving in the apparatus the excess of the *vis viva* which has brought them over that which they carry away with them—that is to say, heat, like an elastic body which rebounds without rising to the height from which it fell.

This heat will be proportional to the square of the mean intensity of the extra current, and will measure it. We shall presently see that it will be feeble if  $\theta=0$ , that it will increase to a maximum for  $\theta=\infty$  (that is, if the bobbin is open). This is in fact what is proved by experiment.

The extra current is produced in the inducing coil as if it were a battery; in returning from the condenser it traverses afresh the wire of this coil. We must ask whether it produces heat. It is easy to answer this question. Suppose the two coils enclosed in the same vessel; they will be traversed by the inducing and the induced currents, the intensities of which are  $I$  and  $I'$ , and which develop quantities of heat

$$C = \frac{bI^2}{\alpha}, \quad C' = \frac{\beta I'^2}{\alpha}.$$

Adding them together, and subtracting the sum from the heat  $w$  observed in this coil, the rest will be due to the extra current. Now we see from the Table that this remainder is virtually nothing.

*Soft Iron.*—We must imagine to ourselves the iron core slowly magnetized as the inducing current increases, very quickly and powerfully during the extra current, that then it becomes slowly demagnetized during the break. It is natural to assume that magnetization creates heat, and demagnetization cold, that these actions have just compensated each other if they have been of equal duration, and that there is a gain of heat the more considerable the shorter has been the first action. If there is any foundation for this view, the extra current should produce in the soft iron a quantity of heat which will increase with it at the same time as  $\theta$ . Experiment proves that this is the case. This heat is represented by the ordinates of an hyperbola; at the limit it attains a maximum.

We have now a very precise idea of induction. The inductor acts both on itself and on the induced wire; it develops with the same facility in both coils an inverse current while it is being established. This is not the case with terminating or direct currents. That of the inducing coil, the extra current, does not pass, or passes with difficulty, in the open circuit; it is compelled to rush into and accumulate in the condenser, which opposes a definite resistance to it. The terminating current of induction, on the contrary, circulates readily through the induced coil if it is closed without any external resistance; the inducing force is thus entirely carried in this direction; there is no extra current, and therefore no heat in the condenser or in the soft iron. If  $\theta$  increases (that is, if a resistance be added to the induced coil and it increases) to infinity, the terminating induced current diminishes gradually until it becomes zero. As the inductive force finds on this side an increasing resistance, it bears more and more upon the inducing coil, and is ultimately entirely expended there. Thus the extra current will increase with

the resistance  $\theta$ , and therefore the heat will increase in the condenser and in the soft iron; for this is the effect of this extra current.

*Inducing Current.*—It is clear that the inducing current transfers the heat into the induced wire, the condenser, and the iron. We are utterly ignorant of the manner in which this transfer is accomplished. It might be that the inducing current preserved its intensity, and lost some of its heat by radiation. But this is not the case: Ohm's formula ceases to be applicable; the current loses force; it preserves the heat which corresponds to its reduced intensity  $I$  (that is to say, a heat equal to the product of  $I^2$  into the resistance  $r$ ); but it has transmitted around it, by a mechanism with which we are unacquainted, and distributed according to the laws which we have just established, the heat corresponding to its lost intensity.

Now, in proportion as  $\theta$  increases, the heat increases in the condenser and in the soft iron; it decreases in the coil  $\beta$ ; it first increases and then diminishes in the resistance  $\theta$ . All this heat is undoubtedly taken from the sum of the thermal units yielded by the pile; what is left disposable is found in the inductor, which must thus vary according to complicated laws. All that can be said and that is justified by experiment is, that the intensity of the inducing current diminishes as  $\theta$  increases, and that it attains a minimum for  $\theta = \infty$ .

*Broken Currents.*—The succession of these phenomena leads us little by little to the case in which  $\theta = \infty$ —that is, to the moment when the induced coil is open, when it gives no more heat, when it has nothing but tensions which are alternately contrary, in which case nothing is expended. This coil may then be neglected or suppressed; everything is reduced to the inducing circuit, to the condenser, and to the soft iron. It is the problem of the broken currents, which continues that of the induction, with which it forms a concordant whole.

1. When  $\theta$  increases to infinity, the intensity of the inducing current diminishes to a minimum. If now the resistance of the coil  $b$  be augmented by an increasing quantity  $t$ , this intensity will continue to decrease according to a very simple law, that of Ohm,

$$I = \frac{nA\alpha}{nR + t},$$

$A$  and  $R$  being generally greater than the electromotive force and the resistance of the  $n$  elements of the pile used.

2. The quantities of heat  $C$  and  $C'$  developed by the current in the resistance  $t$  and in the coil  $b$  are in conformity with Joule's law extended to broken currents,

$$C = \frac{tI^2}{\alpha}, \quad C' = \frac{bI^2}{\alpha}.$$

3. The soft iron has developed a quantity of heat increasing with  $\theta$  to a fixed limit; this heat now decreases progressively and according to Joule's law, as if the soft iron were a wire having the resistance  $f$ :

$$C'' = \frac{fI^2}{\alpha}.$$

4. Like the soft iron, the condenser has developed heat increasing to a fixed limit: like it it gradually loses this heat while  $t$  increases; it is again Joule's law:

$$C''' = \frac{I''^2}{\alpha},$$

putting

$$I'' = \frac{Pa}{Q + t}.$$

The condenser behaves like a resistance equal to unity traversed by a current of the mean intensity  $I''$  which follows Ohm's law. We assume that  $I''$  is the mean intensity of the extra current.—*Comptes Rendus*, August 16, 1869.

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#### ON THE CALORIFIC POWER OF THE LUNAR RAYS.

BY M. H. MARIE-DAVY.

Older experiments made by concentrating, through the aid of mirrors or of lenses, the light of the moon on the most delicate thermometers, gave no appreciable result. Melloni was the first to observe any heating, by concentrating the lunar rays on the face of the pile by means of a glass lens of 3 feet aperture.

Mr. Piazzi Smyth, in the scientific expedition which he undertook in 1856 to the Peak of Teneriffe, confirmed Melloni's experiments. Piazzi Smyth collected the rays directly on the pile, the face of which was provided with the ordinary cone of polished metal. Although the moon was very low, the effect of its rays was a third of that of the rays of a wax candle placed at a distance of 4.75 metres from the pile. A candle placed at the same distance from my pile gave a deflection of  $17^{\circ}.3$  without the introduction of the cone. The direct rays of the moon would therefore have produced a deflection of  $5^{\circ}.8$ , corresponding to 0.00075, if it is possible to compare the flames of candles at such long intervals of time and place.

Lord Rosse, working with a reflector of 3 feet aperture, obtained still more marked results. His thermopile had been previously graduated by being exposed in front of blackened surfaces at definite temperatures. Lord Rosse concludes from his results that the moon's heat-radiation equals that of a surface heated to  $182^{\circ}$  C.

In a Note, of February 18, 1869, on the Heat received from the Stars by the Earth, Mr. Huggins, while establishing the fact of heat from the stars Sirius, Pollux, Regulus, Arcturus, announces that his observations on the full moon were not concordant. On one night a sensible effect was produced; on others, the indications were excessively feeble and were not uniform enough to warrant any confidence being placed in them. Mr. Huggins worked with a refractor of 8 inches, the lenses of which stop almost entirely the obscure heat-rays of the moon, while Lord Rosse's reflector reflects them like the luminous rays.

The moon sends us three kinds of heat-rays—the luminous and obscure rays of the sun reflected or diffused by it, and the rays emanating



directly from its heated surface. These three kinds of rays are combined in Mr. Piazzì Smyth's and Lord Rosse's experiments; I proposed to myself to isolate them as much as possible, and to value them individually, commencing with the luminous rays.

In a preliminary experiment made by means of a differential air-thermometer graduated by comparison with a very delicate mercurial thermometer, I sought the first limit of the phenomenon I wished to measure. The lunar rays concentrated on one of the bulbs of the thermometer by means of an old lens of about 3 feet aperture (belonging to the Imperial Observatory) produced no appreciable effect on the instrument, each division of which corresponds to  $0^{\circ}0043$ . Hence it was necessary to go much further.

I had recourse to the thermopile\*. An astatic galvanometer with long needles, which I owe to the kindness of M. Ruhmkorff and of the Ecole Normale, has enabled me to ascertain almost the hundred thousandth of a degree: one division of the galvanometer is equal to  $0^{\circ}00013$ ; and the tenth of a division can be read off.

The pile was placed behind the ocular of projection of an equatorial of 9 inches, placed in the garden of the Imperial Observatory. The distance from the pile to the ocular was such that the pencil of lunar rays just covered the entire face of the pile. The apparatus, moreover, was protected against external influences by a double metallic envelope, and by four or five folds of black cloth.

The telescope was pointed to the moon so that the image of the satellite given by the seeker covered a circle previously traced on a screen fixed to the apparatus; and the regulator of the equatorial being set in motion, the object-glass was, during a definite number of seconds, alternately opened and shut by a stop, the least disturbance in the direction of the telescope producing a displacement of the needle. Each deflection observed when the stop was removed or replaced was compared with the mean of the two observations immediately before and after. Thus was avoided, as much as possible, the influence of magnetic perturbations, which are always great under an iron cupola. The following Table contains the mean of the results of each series, opposite to the mean of the times of observation. The last column translates these deflections into decimals of a thermometer degree:—

Dates.	Moon's age.	Mean time.	Mean deflection.	Value, in degrees.
		h m		
Oct. 9, 1869.	4th day.	7 32 P.M.	1.3	0.00017
10, „	5th „	7 46	1.0	0.00013
12, „	7th „	8 45	5 8	0.00075
12, „	„ „	9 12	2.2	0.00029
17, „	12th „	8 39	20.0	0.00260
20, „	15th „	10 11	22.1	0.00287

\* It was made with alloys of bismuth and antimony and of antimony and cadmium, the preparation of which has been made known by M. E. Becquerel, and which are endowed with a very high thermoelectric power.



The observations of the 12th were divided into two series. In the second, the moon being very near a misty horizon, the results underwent a very rapid diminution.

On the 20th Mr. Wolf was good enough to take part in the observations, and undertook the readings of the galvanometer. The following are the details of this series, during which the needle was comparatively quiet. I cite it as an example, because, neglecting the first number, obtained before the galvanometer was well in place, it corresponds to the maximum effect observed :—

October 20, 1869. (The scale is inverted.)

Times.	Stop.	Uncorrected deflection.	Mean difference between the effects of the stop on and off.
h m			
9 53	On.	161.5	
56	Off.	120.3	31.5
59	On.	142.1	25.8
10 2	Off.	112.4	23.4
5	On.	131.5	22.8
8	Off.	105.0	26.6
11	On.	131.8	26.0
14	Off.	106.7	19.2
17	On.	120.0	21.2
20	Off.	91.0	23.0
23	On.	108.2	22.5
26	Off.	80.0	22.4
29	On.	96.7	
		Mean ...	22.1

The ratio of the surface of the object-glass to the illuminated surface of the pile was about 230. If we assume that three-fourths of the light which falls on the object-glass reaches the pile, the power of concentration of the telescope is equal to about 247. The deflection obtained on the 20th would then correspond to twelve millionths of a degree for the direct luminous rays of the moon. This is about the sixtieth part of the result obtained by Mr. Piazzi Smyth on the Peak of Teneriffe, working with the whole of the lunar rays.

The luminous heat of the moon increases rapidly with the phase ; but its height and the condition of the sky, even without cloud, greatly affect its intensity.—*Comptes Rendus*, October 25, 1869.

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[FOURTH SERIES.]

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FEBRUARY 1870.

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XII. *On Ocean-currents.* By JAMES CROLL, of the Geological Survey of Scotland\*.

PART I.

*Ocean-currents in relation to the Distribution of Heat over the Globe.*

**T**HE absolute Heating-power of Ocean-currents.—There is perhaps no physical agent concerned in the distribution of heat over the surface of the globe whose influence has been so much underrated as that of ocean-currents. This is no doubt owing to the fact that although a considerable amount of attention has been bestowed in ascertaining the surface-temperature, direction, and general influence of ocean-currents, still little or nothing has been done in the way of determining the absolute amount of heat or of cold conveyed by them, or the absolute increase or decrease of temperature, as the case may be, which must result from the heat or cold conveyed.

The modern method of determining the amount of heat-effects in absolute measure is, no doubt, destined to cast new light on all questions connected with climate, as it has done and is still doing in every department of physics where energy, under the form of heat, is the phenomenon under consideration. But this method has scarcely been attempted yet in questions of meteorology; and owing to the complicated nature of the phenomena with which the meteorologist has generally to deal, its application will very often be found practically impossible. Nevertheless it is particularly suitable to all questions regarding the direct thermal effects of currents, whatever the nature of those currents may happen to be.

\* Communicated by the Author.

In the application of the method to an ocean-current, the two most important elements which we require as data are the volume of the stream and its mean temperature. But although we know something of the temperature of most of the great currents of the ocean, yet, with the exception of the Gulf-stream, little is known regarding the volume of any of them.

Extensive and accurate observations have been made on the breadth, depth, and temperature of the Gulf-stream by the United-States Coast Survey. In the memoirs and charts of the Survey, cross sections of the stream at various places are given, showing its breadth and depth at those places, and also the temperature of the water from the surface downwards to the bottom. We are thus enabled to determine pretty correctly the mean temperature of the stream. And knowing its mean velocity at any given section, we have likewise a means of determining the number of cubic feet of water passing through the section in a given time. But although we can obtain with tolerable accuracy the mean temperature, unfortunately observations regarding the velocity of the water at all depths have not been made at any particular section. Consequently we have no means of estimating so accurately as we should have wished the volume of the current. However, as we know the surface-velocity of the water at places where some of the sections were taken, we are thus enabled to make at least a rough estimate of the volume of the stream.

From an examination of the published sections some years ago\*, I came to the conclusion that the total quantity of water conveyed by the stream is probably equal to that of a stream 50 miles broad and 1000 feet deep†, flowing at the rate of four miles an hour, and that the mean temperature of the entire mass of moving water is not under  $65^{\circ}$  at the moment of leaving the Gulf. I think we are warranted to conclude that the stream, before it returns from its northern journey, is on an average cooled down to at least  $40^{\circ}$ ; consequently it loses  $25^{\circ}$  of heat. Each cubic foot of water, therefore, in this case carries from the tropics for distribution upwards of 1500 units of heat, or 1,158,000 foot-pounds. According to the above estimate of the size and velocity of the stream, 5,575,680,000,000 cubic feet of water are conveyed from the Gulf per hour, or 133,816,320,000,000 cubic feet daily. Consequently the total quantity of heat trans-

\* Philosophical Magazine for February 1867, p. 127.

† The Gulf-stream at the narrowest place examined by the Coast Survey, and the place where its velocity was greatest, was found to be over 30 statute miles broad and 1950 feet deep. But we must not suppose this represents all the warm water which is received by the Atlantic from the equator; a great mass of the water flows into the Atlantic without passing through the Straits of Florida.

ferred from the equatorial regions per day by the stream amounts to 154,959,300,000,000,000 foot-pounds.

This estimate of the volume of the stream is considerably less than that given both by Captain Maury and by Sir John Herschel. Captain Maury considers the Gulf-stream equal to a stream 32 miles broad and 1200 feet deep, flowing at the rate of five knots an hour (*Physical Geography of the Sea*, § 24, 6th edition). This gives 6,165,700,000,000 cubic feet per hour as the quantity of water conveyed by this stream. Sir John Herschel's estimate is still greater. He considers it equal to a stream 30 miles broad and 2200 feet deep, flowing at the rate of four miles an hour (*Physical Geography*, § 54). This makes the quantity 7,359,900,000,000 cubic feet per hour.

From observations made by Sir John Herschel and by M. Pouillet on the direct heat of the sun, it is found that, were no heat absorbed by the atmosphere, about 83 foot-pounds per second would fall upon a square foot of surface placed at right angles to the sun's rays\*. Mr. Meech estimates that the quantity of heat cut off by the atmosphere is equal to about 22 per cent. of the total amount received from the sun. M. Pouillet estimates the loss at 24 per cent. Taking the former estimate, 64.74 foot-pounds per second will therefore be the quantity of heat falling on a square foot of the earth's surface when the sun is in the zenith. And were the sun to remain stationary in the zenith for twelve hours, 2,796,768 foot-pounds would fall upon the surface.

It can be shown that the total amount of heat received upon a unit surface on the equator during the twelve hours from sunrise till sunset at the time of the equinoxes is to the total amount which would be received upon that surface, were the sun to remain in the zenith during those twelve hours, as the diameter of a circle to half its circumference, or as 1 to 1.5708. It follows, therefore, that a square foot of surface on the equator receives from the sun at the time of the equinoxes 1,780,474 foot-pounds daily, and a square mile 49,636,750,000,000 foot-pounds daily. But this amounts to only  $\frac{1}{3,121,870}$  part of the quantity of heat daily conveyed from the tropics by the Gulf-stream. In other words, the Gulf-stream conveys as much heat as is received from the sun by 3,121,870 square miles at the equator. The amount thus conveyed is equal to all the heat which falls upon the globe within 63 miles on each side of the equator. According to calculations made by Mr. Meech†, the annual quantity of heat received by a unit surface on the frigid zone, taking the mean

\* *Trans. of Roy. Soc. of Edinb.* vol. xxi. p. 57. *Phil. Mag.* S. 4. vol. ix. p. 36.

† *Smithsonian Contributions to Knowledge*, vol. ix.

of the whole zone, is  $\frac{5.45}{12}$  of that received at the equator; consequently the quantity of heat conveyed by the Gulf-stream in one year is equal to the heat which falls on an average on 6,873,800 square miles of the arctic regions. The frigid zone or arctic regions contain 8,130,000 square miles. There is actually, therefore, nearly as much heat transferred from tropical regions by the Gulf-stream as is received from the sun by the entire arctic regions, the quantity conveyed by the stream to that received from the sun by those regions being as 15 to 18.

But we have been assuming in our calculations that the percentage of heat absorbed by the atmosphere is no greater in polar regions than it is at the equator, which is not the case. If we make due allowance for the extra amount absorbed in polar regions in consequence of the obliqueness of the sun's rays, the total quantity of heat conveyed by the Gulf-stream will probably nearly equal the amount received from the sun by the entire arctic regions.

If we compare the quantity of heat conveyed by the Gulf-stream with that conveyed by means of ærial currents, the result is equally startling. The density of air to that of water is as 1 to 770, and its specific heat to that of water is as 1 to 4.2; consequently the same amount of heat that would raise 1 cubic foot of water  $1^{\circ}$  would raise 770 cubic feet of air  $4^{\circ}.2$ , or 3234 cubic feet  $1^{\circ}$ . The quantity of heat conveyed by the Gulf-stream is therefore equal to that which would be conveyed by a current of air 3234 times the volume of the Gulf-stream, at the same temperature and moving with the same velocity. Taking, as before, the width of the stream at 50 miles, and its depth at 1000 feet, and its velocity at 4 miles an hour, it follows that, in order to convey an equal amount of heat from the tropics by means of an ærial current, it would be necessary to have a current about  $1\frac{1}{4}$  mile deep, and at the temperature of  $65^{\circ}$ , blowing at the rate of four miles an hour from every part of the equator over the northern hemisphere towards the pole. If its velocity were equal to that of a good sailing-breeze, which Sir John Herschel states to be about twenty-one miles an hour, the current would require to be above 1200 feet deep. A greater quantity of heat is probably conveyed by the Gulf-stream alone from the tropical to the temperate and arctic regions than by all the ærial currents which flow from the equator.

We are apt, on the other hand, to overestimate the amount of the heat conveyed from tropical regions to us by means of ærial currents. The only currents which flow from the equatorial regions are the upper currents, or anti-trades as they are called. But it is not possible that much heat can be conveyed directly by them. The upper currents of the trade-winds, even



at the equator, are nowhere below the snowline; they must therefore lie in a region actually below the freezing-point. In fact, if those currents were warm, they would elevate the snowline above themselves. The heated air rising off the hot burning ground at the equator, after ascending a few miles, becomes exposed to the intense cold of the upper regions of the atmosphere; it then very soon loses all its heat, and returns from the equator much colder than it went thither. It is impossible that we can receive any heat directly from the equatorial regions by means of aerial currents. It is perfectly true that the south-west wind, to which we owe so much of our warmth in this country, is a continuation of the anti-trade; but the heat which this wind brings to us is not derived from the equatorial regions. This will appear evident, if we but reflect that, before the upper current descends to the snowline after leaving the equator, it must traverse a space of at least 2000 miles; and to perform this long journey several days will be required. During all this time the air is in a region below the freezing-point; and it is perfectly obvious that by the time it begins to descend it must have acquired the temperature of the region in which it has been travelling.

If such be the case, it is evident that a wind whose temperature is below  $32^{\circ}$  could never warm a country such as ours, where the temperature does not fall below  $38^{\circ}$  or  $39^{\circ}$ . The heat of our south-west winds is derived, not directly from the equator, but from the warm water of the Atlantic—in fact from the Gulf-stream. The upper current acquires its heat after it descends to the earth. There is one way, however, whereby heat is indirectly conveyed from the equator by the anti-trades; that is, in the form of aqueous vapour. In the formation of one pound of water from aqueous vapour, as Professor Tyndall strikingly remarks, a quantity of heat is given out sufficient to melt five pounds of cast iron\*. It must, however, be borne in mind that the greater part of the moisture of the south-west and west winds is derived from the ocean in temperate regions. The upper current receives the greater part of its moisture after it descends to the earth, whilst the moisture received at the equator is in great part condensed and falls as rain in those regions.

These, as well as many other considerations which might be stated, lead to the conclusion that, in order to raise the mean temperature of the whole earth, water should be placed along the equator, and not land, as is generally believed. For if land is placed at the equator, the possibility of conveying the sun's heat from the equatorial regions by means of ocean-currents is prevented. The transference of heat could then be effected only by

\* Heat as a Mode of Motion, art. 240.

means of the upper currents of the trades ; for the heat conveyed by *conduction* along the solid crust, if any, can have no sensible effect on climate. But these currents, as we have just seen, are ill adapted for conveying heat.

The surface of the ground at the equator becomes intensely heated by the sun's rays. This causes it to radiate off its heat more rapidly into space than a surface of water heated under the same conditions. Again, the air in contact with the hot ground becomes also more rapidly heated than in contact with water, and consequently the ascending current of air carries off a greater amount of heat. But were the heat thus carried away transferred by means of the upper currents to high latitudes and there employed to warm the earth, then it might to a considerable extent compensate for the absence of ocean-currents, and in this case land at the equator might be nearly as well adapted as water for raising the temperature of the whole earth. But such is not the case ; for the heat carried up by the ascending current at the equator is not employed in warming the earth, but is thrown off into the cold stellar space above. This ascending current, instead of being employed in warming the globe, is in reality one of the most effectual means that the earth has of getting quit of the heat received from the sun, and of thus maintaining a much lower temperature than it would otherwise possess. It is in the equatorial regions that the earth loses as well as gains the greater part of its heat ; so that, of all places, here ought to be placed the substance best adapted for preventing the dissipation of the earth's heat into space, in order to raise the general temperature of the earth. Water, of all substances in nature, seems to possess this quality to the greatest extent ; and, besides, it is a fluid, and therefore adapted by means of currents to carry the heat which it receives from the sun to every region of the globe\*.

These results show (although they have reference to only one stream) that the general influence of ocean-currents on the distribution of heat over the surface of the globe must be very great. If the quantity of heat transferred from the equatorial regions by the Gulf-stream alone is nearly equal to all the heat received from the sun by the arctic regions, then how enormous must be the quantity conveyed from the equatorial regions by all the ocean-currents put together !

*Influence of the Gulf-stream on the Climate of Europe.*—In a paper read before the British Association at Exeter, Mr. A. G. Findlay objects to the conclusions at which I have arrived in former papers on the subject, on these grounds, viz. :—1st, that I

\* *Trans. of Glasgow Geol. Soc.* vol. ii. part 3. p. 185 ; *Phil. Mag.* Feb. and June 1867.

nave doubled the actual volume of the stream ; 2nd, that I have not taken into account the great length of time that the water requires in order to circulate, and the interference that it has to encounter in its passage. Although I feel satisfied that the actual quantity of water conveyed from the Gulf of Mexico is as great, if not greater than what I have estimated it to be, yet the influence of the stream on climate, as we shall presently see, is so enormous, that for our present purposes it matters little whether we adopt Mr. Findlay's estimate or mine as the correct one.

In this case I shall adopt Mr. Findlay's estimate, and take the volume of the stream at one-half what I have concluded it to be. The quantity of heat conveyed would still be equal to all the heat received from the sun within  $31\frac{1}{2}$  miles on each side of the equator, or equal to nearly one-half of the entire heat received by the arctic regions.

I may here briefly consider the second objection, as it will afford an opportunity of referring to some important considerations bearing on the mode by which the heat of ocean-currents is distributed over the land.

The objection is, that a stream so comparatively small as the Gulf-stream, after spreading out over such a large area of the Atlantic, and moving so slowly across to the shores of Europe, losing heat all the way, would not be able to produce any very sensible influence on the climate of Europe.

I am unable to perceive the force of this objection. Why, the very efficiency of the stream as a heating-agent necessarily depends upon the slowness of its motion. Did the Gulf-stream move as rapidly along its whole course as it does in the Straits of Florida, it could produce no sensible effect on the climate of Europe. It does not require much consideration to perceive this. (1) If the stream during its course continued narrow, deep, and rapid, it would have little opportunity of losing its heat, and the water would carry back to the tropics the heat which it ought to have given off in the temperate and polar regions. (2) The Gulf-stream does not heat the shores of Europe by direct radiation. Our island, for example, is not heated by radiation from a stream of warm water flowing along its shores. The Gulf-stream heats our island *indirectly* by heating the winds which blow over it to our shores.

The anti-trades, or upper return-currents, as we have seen, bring no heat from the tropical regions. After traversing some 2000 miles in a region of extreme cold they descend on the Atlantic as a cold current, and there absorb the heat and moisture which they carry to north-eastern Europe. Those aerial currents derive their heat from the Gulf-stream, or if it is preferred,

from the warm water poured into the Atlantic by the Gulf-stream.

How, then, are these winds heated by the warm water? The air is heated in two ways, viz. by direct *radiation* from the water, and by *contact* with the water. Now, if the Gulf-stream continued a narrow and deep current during its entire course similar to what it is at the Straits of Florida, it could have little or no opportunity of communicating its heat to the air either by radiation or by contact. If the stream was only about 40 or 50 miles in breadth, the aerial particles in their passage across it would not be in contact with the warm water more than an hour or two. Also the number of the particles in contact with the water, owing to the narrowness of the stream, would be small, and there would therefore be little opportunity for the air becoming heated by contact. The same also holds true in regard to radiation. The more we widen the stream and increase its area, the more we increase its radiating surface; and the greater the radiating surface, the greater is the quantity of heat thrown off. But this is not all; the number of aerial particles heated by radiation increases in proportion to the area of the radiating surface; consequently the wider the area over which the waters of the Gulf-stream are spread, the more effectual will the stream be as a heating-agent. And, again, in order that a very wide area of the Atlantic may be covered with the warm waters of the stream, slowness of motion is essential.

Mr. Findlay says that fully one-half of the Gulf-stream passes eastward and southward from the banks of Newfoundland, and supposes that it is only the northern portion of the current that can be effectual in raising the temperature of Europe. But it appears to me that it is to this south-eastern portion of the current, and not to the northern, that we, in this country, are chiefly indebted for our heat. The south-west winds, to which we owe our heat, derive their temperature from this south-eastern portion which flows away in the direction of the Azores. The south-west winds which blow over the northern portion of the current which flows past our island up into the arctic seas cannot possibly cross this country, but will go to heat Norway and Northern Europe. The northern portion of the stream, no doubt, protects us from the ice of Greenland by warming the north-west winds which come to us from that cold region.

Mr. Buchan, Secretary of the Scottish Meteorological Society, has shown\* that in a large tract of the Atlantic between latitudes  $20^{\circ}$  and  $40^{\circ}$  N., the mean pressure of the atmosphere is greater than in any other place on the globe. To the west of Madeira, between longitude  $10^{\circ}$  and  $40^{\circ}$  W., the mean annual

\* Trans. Roy Soc. of Edinb. vol. xxv. part 2.



pressure amounts to 30·2 inches, while between Iceland and Spitzbergen it is only 29·6, a lower mean pressure than is found in any other place on the northern hemisphere. There must consequently, he concludes, be a general tendency in the air to flow from the former to the latter place along the earth's surface. But the air in moving from the lower to the higher latitudes tends to take a north-easterly direction, and in this case will pass over our island in its course. But it so happens that this region of high pressure is situated in the very path of the south-eastern branch of the Gulf-stream. Consequently the winds blowing from this region of maximum pressure will carry directly to Britain the heat of the Gulf-stream.

It is essential to the heating of our island, as well as the southern portion of Europe, that a very large proportion of the waters of the Gulf-stream should spread over the surface of the Atlantic and never pass up into the arctic regions, as we shall presently see.

But even according to Mr. Findlay's own theory, it is to the south-west wind, heated by the warm waters of the Atlantic, that we are indebted for the high temperature of our climate. But he seems to be under the impression that the Atlantic would be able to supply the necessary heat independently of the Gulf-stream. This, I presume, is the fundamental error of all those who doubt the efficiency of the stream. It is a mistake, however, into which one is very apt to fall who does not adopt the more rigid method of determining heat-results in absolute measure. When we apply this method, we find that the Atlantic without the aid of such a current as the Gulf-stream would be wholly unable to supply the necessary amount of heat to the south-west winds.

The quantity of heat conveyed by the Gulf-stream, as we have seen, is equal to all the heat received from the sun by 3,121,870 square miles at the equator. Mr. Findlay, however, as has been stated, thinks that I have doubled the actual volume of the stream. Assuming that I have done so, the amount of heat carried by the stream would still be equal to all the heat received from the sun by 1,560,935 square miles at the equator. The mean annual quantity of heat received from the sun by the temperate regions per unit surface is to that received by the equator as 9·83 to 12\*. Consequently the quantity of heat conveyed by the stream, taking Mr. Findlay's estimate of its volume, is equal to all the heat received from the sun by 2,062,960 square miles of the temperate regions. The total area of the Atlantic from the latitude of the Straits of Florida, 200 miles north of the tropic of Cancer, up to the Arctic Circle, including also the German Ocean, is about 8,500,000 square miles. In

\* See Smithsonian Contributions to Knowledge, vol. ix.



this case the quantity of heat carried by the Gulf-stream into the Atlantic through the Straits of Florida, to that received by this entire area from the sun, is as 1 to 4·12, or in round numbers as 1 to 4. It therefore follows that one-fifth of all the heat possessed by the waters of the Atlantic over that area, even supposing that they absorb every ray that falls upon them, is derived from the Gulf-stream. Would those who call in question the efficiency of the Gulf-stream be willing to admit that a decrease of one-fourth in the total amount of heat received from the sun, over the entire area of the Atlantic from within 200 miles of the tropical zone up to the arctic region, would not sensibly affect the climate of Northern Europe? If they would not willingly admit this, why, then, contend that the Gulf-stream does not affect climate? for the stoppage of the Gulf-stream, taking it at Mr. Findlay's estimate, would deprive the Atlantic of 77,479,650,000,000,000 foot-pounds of energy in the form of heat per day, a quantity equal to one-fourth of all the heat received from the sun by that area.

How much, then, of the temperature of the south-west winds derived from the water of the Atlantic is due to the Gulf-stream?

Were the sun extinguished, the temperature over the whole earth would sink to nearly that of stellar space, which, according to the investigations of Sir John Herschel\* and of M. Pouillet†, is not above  $-239^{\circ}$  F. Were the earth possessed of no atmosphere, the temperature of its surface would sink to exactly that of space, or to that indicated by a thermometer exposed to no other heat-influence than that of radiation from the stars. But the presence of the atmospheric envelope would slightly modify the conditions of things; for the heat from the stars (which of course constitutes what is called the temperature of space) would, like the sun's heat, pass more freely through the atmosphere than the heat radiated back from the earth, and there would in consequence of this be an accumulation of heat on the earth's surface. The temperature would therefore stand a little higher than that of space; or, in other words, it would stand a little higher than it would otherwise do were the earth exposed in space to the direct radiation of the stars without the atmospheric envelope. But, for reasons which will presently be stated, we may in the mean time, till further light is cast upon this matter, take  $-239^{\circ}$  F. as probably not far from what would be the temperature of the earth's surface were the sun extinguished.

Suppose, now, that we take the mean annual temperature of

\* 'Meteorology,' Section 36.

† *Comptes Rendus*, July 9, 1838. Taylor's *Scientific Memoirs*, vol. iv. p. 44 (1846)

the Atlantic at, say,  $56^{\circ}$  \*. Then  $239^{\circ} + 56^{\circ} = 295^{\circ}$  represents the number of degrees of rise due to the heat which it receives. In other words, it takes all the heat that the Atlantic receives to maintain its temperature  $295^{\circ}$  above the temperature of space. Stop the Gulf-stream, and the Atlantic would be deprived of one-fifth of the heat which it possesses. Then, if it takes five parts of heat to maintain a temperature of  $295^{\circ}$  above that of space, the four parts which would remain after the stream was stopped would only be able to maintain a temperature of four-fifths of  $295^{\circ}$  or  $236^{\circ}$  above that of space: the stoppage of the Gulf-stream would therefore deprive the Atlantic of an amount of heat which would be sufficient to maintain its temperature  $59^{\circ}$  above what it would otherwise be, did it depend alone upon the heat received directly from the sun. It does not, of course, follow that the Gulf-stream actually maintains the temperature  $59^{\circ}$  above what it would otherwise be were there no ocean-currents, because the actual heating-effect of the stream is neutralized to a very considerable extent by cold currents from the arctic regions. But  $59^{\circ}$  of rise represents its actual power; consequently  $59^{\circ}$ , minus the lowering effect of the cold currents, represents the actual rise. What the rise may amount to at any particular place must be determined by other means.

This method of calculating how much the temperature of the earth's surface would rise or fall from an increase or a decrease in the absolute amount of heat received is that adopted by Sir John Herschel in his 'Outlines of Astronomy,' § 369<sup>a</sup>.

About three years ago, in an article in the 'Reader,' I endeavoured to show that this method is not rigidly correct. It has been shown from the experiments of Dulong and Petit, Dr. Balfour Stewart, Professor Draper, and others, that the rate at which a body radiates its heat off into space is not directly proportionate to its absolute temperature. The rate at which a body loses its heat as its temperature rises increases more rapidly than the temperature. As a body rises in temperature the rate at which it radiates off its heat increases; but the *rate* of this increase is not uniform, but increases with the temperature. Consequently the temperature is not lowered in proportion to the decrease of the sun's heat. But at the comparatively low temperature with which we have at present to deal, the error resulting from assuming the decrease of temperature to be proportionate to the decrease of heat would not be great.

It may be observed, however, that the experiments referred to

\* The mean temperature of the Atlantic between the tropics and the arctic circle, according to Admiral FitzRoy's chart, is about  $60^{\circ}$ . But he assigns far too high a temperature for latitudes above  $50^{\circ}$ . It is probable that  $56^{\circ}$  is not far from the truth.

were made on solids ; but, from certain results arrived at by Dr. Balfour Stewart, it would seem that the radiation of a material particle may be proportionate to its absolute temperature. This physicist found that the radiation of a thick plate of glass increases more rapidly than that of a thin plate as the temperature rises, and that, if we go on continually diminishing the thickness of the plate whose radiation at different temperatures we are ascertaining, we find that as it grows thinner and thinner the rate at which it radiates off its heat as its temperature rises becomes less and less. In other words, as the plate grows thinner and thinner its rate of radiation becomes more and more proportionate to its absolute temperature. And we can hardly resist the conviction that if we could possibly go on diminishing the thickness of the plate till we reached a film so thin as to embrace but only one particle in its thickness, its rate of radiation would be proportionate to its temperature. Dr. Balfour Stewart has very ingeniously suggested the probable reason why the rate of radiation of thick plates increases with rise of temperature more rapidly than that of thin. It is this : all substances are more diathermanous for heat of high temperatures than for heat of low temperatures. When a body is at a low temperature, we may suppose that only the exterior rows of particles supply the radiation, the heat from the interior particles being all stopped by the exterior ones, the substance being very opaque for heat of low temperature ; while at a high temperature we may imagine that part of the heat from the interior particles is allowed to pass, thereby swelling the total radiation. But as the plate becomes thinner and thinner, the obstructions to interior radiation become less and less, and as these obstructions are greater for radiation at low temperatures than for radiation at high temperatures, it necessarily follows that, by reducing the thickness of the plate, we assist radiation at low temperatures more than we do at high.

In a gas, where each particle may be assumed to radiate by itself, and where the particles stand at a considerable distance from one another, the obstruction to interior radiation must be far less than in a solid. In this case the rate at which a gas radiates off its heat as its temperature rises must increase more slowly than that of a solid substance. In other words, its rate of radiation must correspond more nearly to its absolute temperature than that of a solid. If this be the case, a reduction in the amount of heat received from the sun, owing to an increase of his distance, should tend to produce a greater lowering effect on the temperature of the air than it does on the temperature of the solid ground. But as the temperature of our climate is determined by the temperature of the air, it must fol-

low that the error of assuming that the decrease of temperature would be proportionate to the decrease in the intensity of the sun's heat may not be great.

It may be noticed here, although it does not bear directly on this point, that although the air in a room, for example, or at the earth's surface is principally cooled by convection rather than by radiation, it is by radiation alone that the earth's atmosphere parts with its heat to stellar space; and this is the chief matter with which we are at present concerned. Air, like all other gases, is a bad radiator; and this tends to protect it from being cooled to such an extent as it would otherwise be, were it a good radiator like solids. True, it is also a bad absorber; but as it is cooled by radiation into space, and heated, not altogether by absorption, but to a very large extent by convection, it on the whole gains its heat more easily than it loses it, and consequently must stand at a higher temperature than it would do were it heated alone by absorption.

But, to return; the error of regarding the decrease of temperature as proportionate to the decrease in the amount of heat received, is probably neutralized by one of an opposite nature, viz. that of taking space at too high a temperature; for by so doing we make the result too small.

We know that absolute zero is at least  $493^{\circ}$  below the melting-point of ice. This is  $222^{\circ}$  below that of space. Consequently, if the heat derived from the stars is able to maintain a temperature of  $-239^{\circ}$ , or  $222^{\circ}$  of absolute temperature, then nearly as much heat is derived from the stars as from the sun. But if so, why do the stars give so much heat and so very little light? If the radiation from the stars could maintain a thermometer  $222^{\circ}$  above absolute zero, then space must be far more transparent to heat-rays than to light-rays, or else the stars give out a great amount of heat, but very little light, neither of which suppositions is probably true. The probability is, I venture to presume, that the temperature of space is not very much above absolute zero. At the time when these investigations into the probable temperature of space were made, at least as regards that by Pouillet, the modern science of heat had no existence, and little or nothing was then known with certainty regarding absolute zero. In this case the whole matter would require to be reconsidered. The result of such an investigation in all probability would be to assign a lower temperature to stellar space than  $-239^{\circ}$ .

Taking all these various considerations into account, it is probable that if we adopt  $-239^{\circ}$  as the temperature of space, we shall not be far from the truth in assuming that the absolute temperature of a place above that of space is proportionate to the amount of heat received from the sun.



We may, therefore, in this case conclude that  $59^{\circ}$  of rise is probably not very far from the truth as representing the influence of the Gulf-stream. The Gulf-stream, instead of producing little or no effect, produces an effect far greater than is generally supposed.

Our island has a mean annual temperature of about  $12^{\circ}$  above the normal due to its latitude. This excess of temperature has been justly attributed to the influence of the Gulf-stream. But it is singular how this excess should have been taken as the measure of the *rise resulting from the influence of the stream*. These figures only represent the number of degrees that the mean normal temperature of our island stands above what is called the normal temperature of the latitude.

The way Professor Dove constructed his Tables of normal temperature was as follows:—He took the temperature of thirty-six equidistant points on every ten degrees of latitude. The mean temperature of these thirty-six points he calls in each case the *normal* temperature of the parallel. The excess above the normal merely represents how much the stream raises our temperature above the mean of all places on the same latitude, but it affords us no information regarding the absolute rise produced. In the Pacific, as well as in the Atlantic, there are immense masses of water flowing from the tropical to the temperate regions. Now, unless we know how much of the normal temperature of a latitude is due to ocean-currents, and how much to the direct heat of the sun, we could not possibly, from Professor Dove's Tables, form the most distant conjecture as to how much of our temperature is derived from the Gulf-stream. The overlooking of this fact has led to a general misconception regarding the positive influence of the Gulf-stream on temperature. The  $12^{\circ}$  marked in Tables of normal temperature do not represent the absolute effect of the stream, but merely how much the stream raises the temperature of our country above the mean of all places on the same latitude. Other places have their temperature raised by ocean-currents as well as this country; only the Gulf-stream produces a rise of several degrees over and above that produced by other streams in the same latitude.

At present there is a difference merely of  $80^{\circ}$  between the mean temperature of the equator and the poles\*; but were each part of the globe's surface to depend alone upon the direct heat which it receives from the sun, there ought, according to theory, to be a difference of more than  $200^{\circ}$ . The annual quantity of

\* The mean temperature of the equator, according to Dove, is  $79^{\circ}7$ , and that of the north pole  $2^{\circ}3$ . But as there is of course some uncertainty regarding the actual mean temperature of the poles, we may take the difference in round numbers at  $80^{\circ}$ .



heat received at the equator to that received at the poles, supposing the proportionate quantity absorbed by the atmosphere to be the same in both cases, is as 12 to 4.98, or, say, as 12 to 5. Consequently, if the temperatures of the equator and the poles be taken as proportionate to the absolute amount of heat received from the sun, then the temperature of the equator above that of space must be to that of the poles above that of space as 12 to 5. What ought, therefore, to be the temperatures of the equator and the poles, did each place depend solely upon the heat which it receives directly from the sun? Were all ocean- and aërial currents stopped, so that there could be no transference of heat from one part of the earth's surface to the other, what ought to be the temperatures of the equator and the poles? We can at least arrive at a rough estimate on this point. If we diminish the quantity of warm water conveyed from the equatorial regions to the temperate and arctic regions, the temperature of the equator will begin to rise and the temperature of the poles to sink. It is probable, however, that this process would affect the temperature of the poles more than it would do that of the equator; for as the warm water flows from the equator to the poles, the area over which it is spread becomes less and less. But as the water from the tropics has to raise the temperature of the temperate regions as well as the polar, the difference of effect at the equator and poles might not, on that account, be so very great. Let us take a rough estimate. Say that, as the temperature of the equator rises one degree, the temperature of the poles sinks one degree and a half. The mean annual temperature of the globe is about  $58^{\circ}$ . The mean temperature of the equator is  $80^{\circ}$ , and that of the poles  $0^{\circ}$ . Let ocean- and aërial currents now begin to cease, the temperature of the equator begins to rise and the temperature of the poles to sink. For every degree that the equator rises the poles sink  $1\frac{1}{2}^{\circ}$ ; and when the currents are all stopped and each place dependent alone upon the direct rays of the sun, the mean annual temperature of the equator above that of space will be to that of the poles, above that of space, as 12 to 5. When this proportion is reached, the equator will be  $374^{\circ}$  above that of space, and the poles  $156^{\circ}$ ; for 374 is to 156 as 12 is to 5. The temperature of space we have seen to be  $-239^{\circ}$ , consequently the temperature of the equator will in this case be  $135^{\circ}$ , reckoned from the zero of the Fahrenheit thermometer, and the poles  $83^{\circ}$  below zero. The equator would therefore be  $55^{\circ}$  warmer than at present, and the poles  $83^{\circ}$  colder. The difference between the temperature of the equator and the poles will in this case amount to  $218^{\circ}$ .

Now, if we take into account the quantity of positive energy

in the form of heat carried by warm currents from the equator to the temperate and polar regions, and also the quantity of negative energy (cold) carried by cold currents from the polar regions to the equator, we shall find that they are sufficient to reduce the difference of temperature between the poles and the equator from  $218^{\circ}$  to  $80^{\circ}$ .

The quantity of heat received in the latitude of London, for example, to that received at the equator is about as 12 to 8. This, according to theory, should produce a difference of about  $125^{\circ}$ . The temperature of the equator above that of space, as we have seen, would be  $374^{\circ}$ . Therefore  $249^{\circ}$  above that of space would represent the temperature of the latitude of London. This would give  $10^{\circ}$  as its temperature. The stoppage of all ocean and ærial currents would thus increase the difference between the equator and the latitude of London by about  $85^{\circ}$ . The stoppage of ocean-currents would not be nearly so much felt, of course, in the latitude of London as at the equator and the poles, because, as has been already noticed, in all latitudes midway between the equator and the poles the two sets of currents to a considerable extent compensate each other; viz. the warm currents from the equator raise the temperate, while the cold ones from the poles lower it; but as the warm currents chiefly keep on the surface and the cold return-currents are principally undercurrents, the heating effect very greatly exceeds the cooling effect. Now, as we have seen, the stoppage of all currents would raise the temperature of the equator  $55^{\circ}$ ; that is to say, the rise at the equator alone would increase the difference of temperature between the equator and that of London by  $55^{\circ}$ . But the actual difference, as we have seen, ought to be  $85^{\circ}$ ; consequently the temperature of London would be lowered  $30^{\circ}$  by the stoppage of the currents. For if we raise the temperature of the equator  $55^{\circ}$  and lower the temperature of London  $30^{\circ}$ , we then increase the difference by  $85^{\circ}$ . The normal temperature of the latitude of London being  $40^{\circ}$ , the stoppage of all ocean and ærial currents would thus reduce it to  $10^{\circ}$ . But the Gulf-stream raises the actual mean temperature of London  $10^{\circ}$  above the normal. Consequently  $30^{\circ} + 10^{\circ} = 40^{\circ}$  represents the actual rise at London due to the influence of the Gulf-stream over and above all the lowering effects resulting from arctic currents. On some parts of the American shores on the latitude of London, the temperature is  $10^{\circ}$  below the normal. The stoppage of all ocean- and ærial currents would therefore lower the temperature there only  $20^{\circ}$ .

It is at the equator and the poles that the great system of ocean and ærial currents produces its maximum effects. The influence becomes less and less as we recede from those places,

and between them there is a point where the influence of the warm currents from the equator and cold currents from the poles exactly neutralize each other. At this point the stoppage of ocean-currents would not sensibly affect temperature. This point, of course, is not situated on the same latitude in all meridians, but varies according to the position of the meridian in relation to land, and ocean-currents, whether cold or hot, and other circumstances. A line drawn round the globe through these various points would be very irregular. At one place, such as on the western side of the Atlantic, where the arctic current predominates, the neutral line would be deflected towards the equator, while on the eastern side, where warm currents predominate, the line would be deflected towards the north. It is a difficult problem to determine the mean position of this line; it probably lies somewhere not far north of the tropics.

*Influence of the Gulf-stream on the Climate of the Arctic Regions.*—Does the Gulf-stream pass into the arctic regions? Are the seas around Spitzbergen and North Greenland heated by the warm water of the stream?

Those who deny this nevertheless admit the existence of an arctic current. They admit that an immense mass of cold water is continually flowing south from the polar regions around Greenland into the Atlantic. Then, if it is admitted that there is a mass of water flowing across the arctic circle from north to south, it must also be admitted that there is an equal mass flowing across from south to north. It is also evident that the water crossing from south to north must be warmer than the water crossing from north to south; for the temperate regions are warmer than the arctic, and the ocean in temperate regions warmer than the ocean in the arctic; consequently the current which flows into the arctic seas, to compensate for the cold arctic current, must be a warmer current.

Is the Gulf-stream this warm current? Does this compensating warm current proceed from the Atlantic or from the Pacific? If it proceeds from the Atlantic, it is simply the warm water of the Gulf-stream. We may call it the warm water of the Atlantic if we choose; but this cannot materially affect the question at issue, for the heat which the waters of the Atlantic possess is derived, as we have seen, to an enormous extent from the water brought from the tropics by the Gulf-stream. Then, if we deny that the warm compensating current comes from the Atlantic, we must assume that it comes from the Pacific. But if the cold current flows from the arctic regions into the Atlantic, and the warm compensating current from the Pacific into the arctic regions, the highest temperature should be found on the Pacific side of the arctic regions and not on the Atlantic side;

but the reverse is the case. In the Atlantic, for example, the  $41^{\circ}$  isothermal line of mean annual temperature reaches to latitude  $65^{\circ} 30'$ , while in the Pacific it nowhere goes beyond latitude  $57^{\circ}$ . The  $27^{\circ}$  isothermal of mean annual temperature reaches to latitude  $75^{\circ}$  in the Atlantic, but in the Pacific it does not pass beyond  $64^{\circ}$ . And the  $14^{\circ}$  isothermal reaches the north of Spitzbergen in latitude  $80^{\circ}$ , whereas on the Pacific side of the arctic regions it does not reach to latitude  $72^{\circ}$ .

On no point of the earth's surface does the mean annual temperature rise so high above the normal as in the northern Atlantic, just at the arctic circle, at a spot believed to be in the middle of the Gulf-stream. This place is no less than  $22^{\circ} \cdot 5$  above the normal, while in the northern Pacific the temperature does not anywhere rise more than  $9^{\circ}$  above the normal. These facts prove that the warm current passes up the Atlantic into the arctic regions and not up the Pacific, or at least that the larger amount of warm water must pass into the arctic regions through the Atlantic. In other words, the Gulf-stream is the warm compensating current. Not only must there be a warm stream, but one of very considerable magnitude, in order to compensate for the great amount of cold water that is constantly flowing from the arctic regions, and also to maintain the temperature of those regions so much above the temperature of space as they actually are.

No doubt, when the results of the late dredging expedition in the North Sea are published, they will cast much additional light on the direction and character of the currents forming the north-eastern branch of the Gulf-stream.

The average quantity of heat received by the arctic regions as a whole per unit surface to that received at the equator, as we have already seen, is as  $5 \cdot 45$  to 12, assuming that the percentage of rays cut off by the atmosphere is the same at both places. In this case the mean annual temperature of the arctic regions, taken as a whole, would be about  $-69^{\circ}$ , did those regions depend alone for their temperature upon the heat received directly from the sun. But the temperature would not even reach to this; for the percentage of rays cut off by the atmosphere in arctic regions is generally believed to be greater than at the equator, and consequently the actual mean quantity of heat received by the arctic regions will be less than  $\frac{5 \cdot 45}{12}$  of what is received at the equator.

In the article on "Climate" in the *Encyclopædia Britannica* there is a Table calculated upon the principle that the quantity of heat cut off is proportionate to the number of aerial particles that the rays have to encounter before reaching the surface of the earth—that, as a general rule, if the tracts of the rays fol-



low an arithmetical progression, the diminished force with which the rays reach the ground will form a decreasing geometrical progression. According to this Table about 75 per cent. of the sun's rays are cut off by the atmosphere in arctic regions. But if 75 per cent. of the rays were cut off by the atmosphere in arctic regions, then the direct rays of the sun could not maintain a mean temperature  $100^{\circ}$  above that of space. But this is no doubt by far too high a percentage for the quantity of heat cut off; for recent discoveries in regard to the absorption of radiant heat by gases and vapours prove that Tables computed on this principle must be incorrect. The researches of Tyndall and Melloni show that when rays pass through any substance, the absorption is rapid at first; but the rays are soon "sifted," as it is called, and they then pass onwards with but little further obstruction. Still, however, owing to the dense fogs which prevail in arctic regions, the quantity of heat cut off must be considerable. If as much as 50 per cent. of the sun's rays are cut off by the atmosphere in arctic regions, the amount of heat received directly from the sun is not sufficient to maintain a mean annual temperature of  $-100^{\circ}$ . Consequently the arctic regions must depend to an enormous extent upon ocean-currents for their temperature.

*Influence of Ocean-currents shown by a Second method.*—That the temperature of the arctic regions would sink enormously and the temperature of the equator rise enormously were all ocean-currents stopped, can be shown by another method, viz. by taking the mean annual temperature from the equator to the pole along a meridian passing through the ocean, say, the Atlantic, and comparing it with the mean annual temperature taken along a meridian passing through a great continent, say, the Asiatic.

Professor J. D. Forbes, in an interesting memoir\*, has endeavoured by this method to determine what would be the temperature of the equator and the poles were the globe all water or all land. He has taken the temperature of the two meridians from the Tables and charts of M. Dove, and ascertained the exact proportion of land and water on every  $10^{\circ}$  of latitude from the equator to the poles, with the view of determining what proportion of the average temperature of the globe in each parallel is due to the land, and what to the water which respectively belongs to it. He next endeavours to obtain a formula for expressing the mean temperature of a given parallel, and thence arrives at "an approximate answer to the inquiry as to what would have been the equatorial or polar temperature of the globe, or that of any latitude, had its surface been entirely composed of land or water."

\* Trans. of Roy. Soc. Edinb. vol. xxii. p. 75.



The result at which he arrived is this : that, were the surface of the globe all water,  $71^{\circ}7$  would be the temperature of the equator and  $12^{\circ}5$  the temperature of the poles ; and were the surface all land,  $109^{\circ}8$  would be the temperature of the equator and  $-25^{\circ}6$  the temperature of the poles.

But in Professor Forbes's calculations no account whatever is taken of the influence of currents, whether of water or of air, and the difference of temperature is attributed wholly to difference of latitude and the physical properties of land and water in relation to their powers in absorbing and detaining the sun's rays, and to the laws of conduction and of convection which regulate the internal motion of heat in the one and in the other. He considers that the effects of currents are all compensatory.

"If a current of hot water," he says, "moderates the cold of a Lapland winter, the countercurrent, which brings the cold of Greenland to the shores of the United States, in a great measure restores the balance of temperature, so far as it is disturbed by this particular influence. The prevalent winds, in like manner, including the trade-winds, though they render some portions of continents, on the average, hotter or colder than others, produce just the contrary effect elsewhere. Each continent, if it has a cold eastern shore, has likewise a warm western one ; and even local winds have for the most part established laws of compensation. In a given parallel of latitude all these secondary causes of local climate may be imagined to be mutually compensatory, and the outstanding gradation of mean or normal temperature will mainly depend, 1st, upon the effect of latitude simply, 2nd, on the distribution of land and water considered in their primary or *statical* effect."

It is singular that a physicist so acute as Professor Forbes should, in a question such as this, leave out of account the influence of currents, under the impression that their effects were compensatory.

If there is a constant transference of hot water from the equatorial regions to the polar, and of cold water from the polar regions to the equatorial (a thing which Professor Forbes admitted), then there can only be one place between the equator and the pole where the two sets of currents compensate each other. At all places on the equatorial side of this point a cooling effect is the result. Starting from this neutral point, the preponderance of the cooling effect over the heating increases as we approach towards the equator, and the preponderance of the heating effect over the cooling increases as we recede from this point towards the pole—the cooling effect reaching a maximum at the equator, and the heating effect a maximum at the pole.

Had Professor Forbes observed this important fact, he would

have seen at once that the low temperature of the land in high latitudes in comparison with that of the sea was no index whatever as to how much the temperature of those regions would sink were the sea entirely removed and the surface to become land ; for the present high temperature of the sea is not due wholly to the mere physical properties of water, but to a great extent is due to the heat brought by currents from the equator. Now, unless it is known how much of the absolute temperature of the ocean in those latitudes is due to currents, we cannot tell how much the removal of the sea would lower the absolute temperature of those places. Were the sea removed, the continents in high latitudes would not simply lose the heating advantages which they presently derive from the mere fact of their proximity to so much sea, but the removal would, in addition to this, deprive them of an enormous amount of heat which they at present receive from the tropics by means of ocean-currents. And, on the other hand, at the equator, were the sea removed, the continents there would not simply lose the cooling influences which result from their proximity to so much water, but, in addition to this, they would have to endure the scorching effects which would result from the heat which is at present carried away from the tropics by ocean-currents.

We have already seen that Professor Forbes concluded that the removal of the sea would raise the mean temperature of the equator  $30^{\circ}$  and lower the temperature of the poles  $28^{\circ}$  ; it is therefore perfectly certain that, had he added to his result the effect due to ocean-currents, and had he been aware that about one-fifth of all the heat possessed by the Atlantic is actually derived from the equator by means of the Gulf-stream, he would have assigned a temperature to the equator and the poles, of a globe all land, differing not very far from what I have concluded would be the temperature of those places were all ocean and ærial currents stopped, and each place to depend solely upon the heat which it directly received from the sun.

*Influence of Ocean-currents shown by a Third method.*—The great influence of ocean-currents in lowering the temperature of the equator and raising the temperature of the poles can also be shown in still another way.

The general influence of land on climate is to exaggerate the variation of temperature due to the seasons. On continents the summers are hotter and the winters colder than on the ocean. The days are also hotter and the nights colder on land than on sea. This is a result which follows from the mere physical properties of land and water, independently of currents, whether of ocean or of air. But it nevertheless follows, according to theory (and this is a point which has been overlooked), that the mean

annual temperature of the ocean ought to be greater than that of the land in equatorial regions as well as in temperate and polar regions. This will appear obvious for the following reasons:—

(1) The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles and its transparency for heat-rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is comparatively small; the quantity stored up in the ocean is great. (2) The air is probably heated more rapidly by contact with the ground than with the ocean; but, on the other hand, it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapour of the air is to a great extent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated. (3) The air radiates back a considerable portion of its heat, and the ocean absorbs this radiation from the air more readily than the ground does. The ocean will not reflect the heat from the aqueous vapour of the air, but absorbs it, while the ground does the opposite. Radiation from the air, therefore, tends more readily to heat the ocean than it does the land. (4) The aqueous vapour of the air acts as a screen to prevent the loss by radiation from water, while it allows radiation from the ground to pass more freely into space; the atmosphere over the ocean consequently throws back a greater amount of heat than is thrown back by the atmosphere over the land. The sea in this case has a much greater difficulty than the land has in getting quit of the heat received from the sun; in other words, the land tends to lose its heat more rapidly than the sea. The consequence of all these circumstances is that the ocean must stand at a higher mean temperature than the land. A state of equilibrium is never gained until the rate at which a body is receiving heat is equal to the rate at which it is losing it; but as equal surfaces of sea and land receive from the sun the same amount of heat, it therefore follows that, in order that the sea may get quit of its heat as rapidly as the land, it *must stand at a higher temperature* than the land. The temperature of the sea must continue to rise till the amount of heat thrown off into space equals that received from the sun; when this point is reached, equilibrium is established and the temperature remains stationary. But, owing to the greater difficulty that the sea has in getting rid of its heat, the mean temperature of equilibrium of the ocean must be higher than that of the land; consequently the mean temperature of the ocean, and also of the air immediately over it, in tropical regions should be higher than the mean temperature of the land and the air over it.

We find, however, when we examine M. Dove's charts of

mean annual temperature, that the reverse is actually the case ; for the ocean in tropical regions has a mean annual temperature below the normal, and the land a mean annual temperature above the normal. Both in the Pacific and in the Atlantic the mean temperature sinks to  $2^{\circ}3$  below the normal, while on the land it rises  $4^{\circ}6$  above the normal. This can be accounted for upon no other principle than that the ocean in tropical regions is kept at a lower temperature in consequence of the enormous amount of *heat* that is being constantly carried away from those regions into temperate and polar regions, and of *cold* that is being constantly carried from temperate and polar regions to the tropical regions by means of ocean-currents.

Were there no ocean-currents, it would follow, according to theory, that the southern hemisphere should be warmer than the northern, because the proportion of sea to land is greater on that hemisphere than on the northern ; but we find that the reverse is the case.

*Low Temperature of the Southern Hemisphere owing to Ocean-currents.*—The lower mean temperature of the southern hemisphere is due to the amount of heat transferred over from that hemisphere to the northern by ocean-currents.

The great ocean-currents of the globe take their rise in three immense streams from the Southern Ocean, which, on reaching the tropical regions, become deflected in a westerly direction and flow along the southern side of the equator for thousands of miles. Perhaps more than one-half of this mass of moving water returns into the Southern Ocean without ever crossing the equator ; but the quantity which crosses over to the northern hemisphere is enormous. Since there is then a constant flow of water from the southern hemisphere to the northern in the form of surface-currents, it must be compensated by *undercurrents* of equal magnitude from the northern hemisphere to the southern. The currents, however, which cross the equator are far higher in temperature than their compensating undercurrents ; consequently there is a constant transference of heat from the southern hemisphere to the northern. Any currents taking their rise in the northern hemisphere and flowing across into the southern are comparatively trifling, and the amount of heat transferred by them is also trifling. There are one or two currents of considerable size, such as the Brazilian branch of the great equatorial current of the Atlantic, and a part of the South Equatorial Drift-current of the Pacific, which cross the equator from north to south ; but these cannot be regarded as northern currents ; they are simply southern currents deflected back after crossing over to the northern hemisphere. The heat which these currents possess is chiefly obtained on the southern hemisphere before



crossing over to the northern ; and although the northern hemisphere may not gain much heat by means of them, it, on the other hand, does not lose much, for the heat which they give out in their progress along the southern hemisphere does not belong to the northern hemisphere.

But, after making the fullest allowance for the amount of heat carried across the equator from the northern hemisphere to the southern, we shall find, if we compare the mean temperature of the currents from the southern hemisphere to the northern with the mean temperature of the great compensating undercurrents and the one or two small surface-currents, that the mean temperature of the water crossing the equator from the southern hemisphere to the northern is very much higher than the mean temperature of the water crossing from the northern hemisphere to the southern. The mean temperature of the water crossing the equator from south to north is probably not under  $65^{\circ}$ , while the mean temperature of the undercurrents is probably not over  $39^{\circ}$ . But we must add to them the surface-currents from north to south. And let us assume that this will raise the mean temperature of the entire mass of water flowing from north to south to, say,  $45^{\circ}$ . Supposing this to be a fair estimate, here we have a difference of  $20^{\circ}$ . Each cubic foot of water which crosses the equator will in this case transfer about 1250 units of heat from the southern hemisphere to the northern. If we had any means of ascertaining the volume of those great currents crossing the equator, we should then be able to make a rough estimate of the total amount of heat transferred from the southern hemisphere to the northern ; but as yet no accurate estimate has been made on this point. Let us assume, what is probably below the truth, that the total amount of water crossing the equator is at least double that of the Gulf-stream as it passes through the Straits of Florida, which amount we have already found to be equal to 133,816,320,000,000 cubic feet daily. Taking the quantity of heat conveyed by each cubic foot of water of the Gulf-stream as 1500 thermal units, it is found, as we have seen, that an amount of heat is conveyed by this current equal to all the heat that falls within 63 miles on each side of the equator. Then, if each cubic foot of water crossing the equator transfers 1250 thermal units, and the quantity of water is double that of the Gulf-stream, it follows that the amount of heat transferred from the southern hemisphere to the northern is equal to all the heat falling within 105 miles on each side of the equator, or equal to all the heat falling on the southern hemisphere within 210 miles of the equator. This quantity taken from the southern hemisphere and added to the northern will therefore make a difference in the amount of heat possessed by the two hemispheres

equal to all the heat which falls on the southern hemisphere within fully 420 miles of the equator, supposing the sun to be vertical over the whole area. But even supposing that the actual difference is only one-half this amount, still it is nearly equal to all the heat which is received by the entire area of the Atlantic Ocean from the tropics to the arctic regions, a quantity quite sufficient to account for the lower mean temperature of the southern hemisphere.

*Without Ocean-currents the Globe would not be habitable.*—All these foregoing considerations show to what an extent the climatic condition of our globe is due to the thermal influences of ocean-currents.

As regards the northern hemisphere, we have two immense oceans, the Pacific and the Atlantic, extending from the equator to near the north pole, or perhaps to the pole altogether. Between these two oceans lie two great continents, the eastern and the western. Owing to the earth's spherical form, by far too much heat is received at the equator and by far too little at high latitudes to make the earth a suitable habitation for sentient beings. The function of these two great oceans is to remove the heat from the equator and carry it to temperate and polar regions. Aërial currents could not do this. They might remove the heat from the equator, but they could not, as we have already seen, carry it to the temperate and polar regions; for the greater portion of the heat which aërial currents remove from the equator is dissipated into stellar space: the ocean can alone convey the heat to distant shores. But aërial currents have a most important function; for of what avail would it be, though ocean-currents should carry heat to high latitudes, if there were no means of spreading the heat thus conveyed over the land? The function of aërial currents is to do this. Upon this twofold arrangement depends the thermal condition of the globe. Exclude the waters of the Pacific and the Atlantic from temperate and polar regions and place them at the equator, and nothing now existing on the globe could live in high latitudes.

Were these two great oceans placed beside each other on one side of the globe, and the two great continents placed beside each other on the other side, the northern hemisphere would not then be suitable for the present order of things: the land on the central and on the eastern side of the united continent would be by far too cold.

*The foregoing conclusions not affected by the imperfection of the data.*—It will hardly do to object to the general results at which we have arrived in reference to the influence of ocean currents on the climatic condition of the globe on the ground of any imperfection in the data employed. It is perfectly true that con-

siderable uncertainty prevails regarding some of the data ; but, after making the fullest allowance for every possible error, the influence of currents is so enormous that the general conclusion cannot be materially affected. I can hardly think that any one familiar with the physics of the subject would be willing to admit that it is probable that, owing to errors in the data, the effects have been doubled. But even admitting that they have, still it does not materially alter the general conclusion at which we have arrived. The influence of ocean-currents in the distribution of heat over the surface of the globe must still be admitted to be enormous, whether we conclude that owing to them the present temperature of the equator is  $55^{\circ}$  or  $27^{\circ}$  colder than it would otherwise be, or the poles  $83^{\circ}$  or  $41^{\circ}$  hotter than they would be were there no currents.

Yea, more, suppose we should again halve the result ; still we should have to admit that, owing to ocean-currents, the equator is about  $14^{\circ}$  colder and the poles about  $21^{\circ}$  hotter than they would otherwise be ; in other words, we should have to admit that, were it not for ocean-currents, the mean temperature of the equator would be about  $100^{\circ}$  and the mean temperature of the poles about  $-21^{\circ}$ .

If the influence of ocean-currents in reducing the difference between the temperature of the equator and poles amounted to only a few degrees, it would, of course, be needless to put much weight on any results arrived at by the method of calculation which has been adopted in this paper ; but when it is a matter of two hundred degrees, it is not at all likely that the general results will be very much affected by any errors which may ever be found in the data.

Objections of a palæontological nature have frequently been urged against the opinion that our island is much indebted for its mild climate to the influence of the Gulf-stream ; but, from what has already been stated, it must, I presume, be apparent that all objections of that nature are of little avail. The palæontologist may detect, from the character of the flora and fauna brought up from the sea-bottom by dredging and other means, the presence of a warm or of a cold current ; but this can never enable him to prove that the temperate and polar regions are not affected to an enormous extent by warm water conveyed from the equatorial regions. For anything that palæontology can show to the contrary, were ocean-currents to cease, the mean annual temperature of our island might sink below the present midwinter temperature of Siberia. What would be the thermal condition of our globe were there no ocean-currents is a question for the physicist ; not for the naturalist.

XIII. *Note on an Elementary Proof of a Theorem of Lagrange's.*  
By Professor ROBERT STAWELL BALL, A.M.\*

“ IF a membrane, extensible or inextensible, be in equilibrium under the influence of forces at every point normal to the surface, then the normal pressure at any point is equal to the tension of the surface at that point, multiplied into the sum of the reciprocals of the principal radii of curvature.”

This theorem is at present of great interest, in consequence of its very beautiful application in the recent discoveries of Professor Haughton.

Taking the origin on the surface, the axis of  $z$  for the normal, and the axes of  $x$  and  $y$  tangents to the principal directions of curvature, the equation of the surface is

$$2z - \frac{x^2}{R_1} - \frac{y^2}{R_2}$$

+ terms of the third and higher orders = 0.

But as we shall only deal with points in the immediate vicinity of the origin, we may neglect all but the three first terms, and thus

$$2z - \frac{x^2}{R_1} - \frac{y^2}{R_2} = 0.$$

Now, let a plane parallel to the tangent-plane, and separated from it by an infinitely small interval  $p$ , be drawn; this will cut the surface in an ellipse defined by the equations

$$z = p,$$

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1,$$

if

$$a^2 = 2R_1 p \text{ and } b^2 = 2R_2 p.$$

Let us fix our attention on the small piece of the surface thus cut off. It may be considered plane, because the greatest deviation of any part of it from the plane is of the second order of small quantities. Hence, if  $P$  be the normal pressure per unit of area to which the surface is at this point exposed, the total pressure on the piece bounded by the ellipse is

$$\pi abP.$$

This pressure is counteracted by the tension of the surface which acts along the margin of the ellipse.

Let  $\lambda$  be the tension of the membrane per unit of length, then

\* Communicated by the Author.



the tension along a length  $ds$  is

$$\lambda ds.$$

Around the circumference of the ellipse the tension of any element of the arc acts in the tangent-plane and perpendicular to the arc. The direction-cosines of the normal at the point  $xyz$  are

$$-\frac{x}{R_1}, \quad -\frac{y}{R_2}, \quad 1,$$

and the direction-cosines of the element of the arc of the ellipse are

$$\frac{dx}{ds}, \quad \frac{dy}{ds}, \quad 0.$$

Hence, since these lines are perpendicular, the cosine of the angle which a line that is perpendicular to both of them makes with the axis of  $z$  is

$$\frac{y}{R_2} \cdot \frac{dx}{ds} - \frac{x}{R_1} \cdot \frac{dy}{ds}.$$

The tension of the element  $ds$  is to be resolved along the axis of  $z$  by multiplying  $\lambda ds$  by this quantity, the product being

$$\lambda \left( \frac{y}{R_2} \cdot dx - \frac{x}{R_1} \cdot dy \right).$$

Now, according to Lagrange's notion of the nature of such a membrane,  $\lambda$  is constant around this ellipse.

Let then  $x = a \sin \phi, \quad y = b \cos \phi.$

The entire tension of the surface along the ellipse resolved along the axis of  $z$  is

$$\begin{aligned} 4\lambda ab \int_0^{\frac{\pi}{2}} \left( \frac{1}{R_2} \cos^2 \phi + \frac{1}{R_1} \sin^2 \phi \right) \cdot d\phi \\ = \pi \lambda ab \left( \frac{1}{R_1} + \frac{1}{R_2} \right); \end{aligned}$$

but this must be equal to the normal pressure on the ellipse, which is

$$\pi abP,$$

hence

$$\lambda \left( \frac{1}{R_1} + \frac{1}{R_2} \right) = P. \quad \text{Q. E. D.}$$

XIV. *On the determination of the size of the Wire which, wound upon a Galvanometer or Electromagnet, will produce the maximum magnetic effect in a circuit of given external resistance, the space taken up by the silk or other substance insulating the different convolutions from each other being taken into consideration.* By G. K. WINTER, *Telegraph Engineer, Madras Railway Company*\*.

[With a Plate.]

IT is easily proved, and generally known, that if it were not necessary to insulate the different convolutions from each other, or if the ratio between the space taken up by the insulating covering and that occupied by the wire itself were constant for all sizes of wire, the maximum magnetic effect would be produced when the resistance of the galvanometer or electromagnet coil is equal to the sum of the external resistances in the circuit. It is evident, however, that the ratio above mentioned varies with the size of the wire; the thickness of the silk covering being the same, or nearly the same, for all sizes of wire.

Mr. Schwendler, in a paper in the *Philosophical Magazine* for January 1867, taking the silk covering into account, proved that the resistance of the coil should be less than the external resistance, and gave formulæ for determining what the coil resistance should be in order to produce the maximum effect.

It is my purpose in the following investigation, not to calculate directly what the resistance of the coil should be, but to determine the size of the wire when the magnetic effect is a maximum; and I trust the result will prove of practical value to electricians.

In winding a magnet or galvanometer coil, the wire may either be wound in layers alternately backwards and forwards, or it may be wound in superposed layers, the end of each layer being connected with the commencement of the next; or if greater insulation be required, each layer is sometimes separated from the next above it by a layer of paper, gutta-percha tissue, or other insulating substance.

Plate II. figs. 1, 2, 3 represent sections of the coil with the wire wound in the three methods above mentioned, and will render what follows more easily understood.

Space is undoubtedly best economized in the second of these methods (fig. 2)†; but, unfortunately, in order to coil the wire in this way, all the layers must be wound in the same direction;

\* Communicated by the Author.

† This would be more evident if the size of the wire in the diagram were not necessarily so exaggerated in proportion to the size of the coil.

and this is only rendered practicable by joining the end of one layer to the beginning of the next, which is necessarily at the other end of the coil. This may be done either by making the connexion outside the coil (which is only possible when the wire is thick and the layers consequently not very numerous), or the wire may be carried across over the layer just wound; but the economy of this plan is doubtful, and it is rarely adopted; it introduces extra resistance into the circuit, and space is lost by the wire in the next layer having to bridge over the connecting wire.

Let  $r$  = the radius of the wire,  
 $s$  = the thickness of the silk or other insulator,  
 $m$  = the sectional area of the space to be filled with wire,  
 $l$  = the average length of one convolution,  
 $c$  = the specific conductivity of the wire to be used, that of pure copper at  $0^\circ$  Centigrade being unity, and  
 $\rho$  and  $\lambda$  = respectively the radius and length of a wire of pure copper equal in resistance to the given external resistance.

It is at once evident that the sectional area taken up by each convolution, if the wire be wound according to the first method (fig. 1), will be  $4(r+s)^2$ .

It is also equally evident that the sectional area taken up by each convolution, if the second method of winding the wire be adopted, will be

$$2(r+s) \sqrt{4(r+s)^2 - (r+s)^2} = 2 \sqrt{3}(r+s)^2,$$

in which the coefficient 4 is replaced by  $2\sqrt{3} = 3.4641$ . This coefficient, which depends entirely upon the method of coiling the wire, we will call  $a$ , either value being substituted for it in the numerical solution of the problem, according to the circumstances of the case.

With regard to the third method of winding the wire (fig. 3), namely in layers insulated from each other by a layer of paper or other insulator, it will be sufficiently correct for practical purposes if we consider  $s$  increased by one quarter of the thickness of the insulating layer.

The sectional area taken up by each convolution being  $a(r+s)^2$ , the number of convolutions will be  $\frac{m}{a(r+s)^2}$ .

The area of the copper wire will be  $r^2\pi$ ; hence the resistance of the galvanometer  $= \frac{lm}{r^2\pi a(r+s)^2}$ .

$c\lambda$  will be the length of a wire of the same conductivity as that

used in winding the galvanometer, which, if the radius be  $\rho$ , will equal in resistance the given external resistance, which will be equal consequently to  $\frac{c\lambda}{\rho^2\pi}$ .

The magnetic effect will be

$$\frac{\frac{m}{a(r+s)^2}}{\frac{c\lambda}{\rho^2\pi} + \frac{lm}{r^2\pi a(r+s)^2}} = \frac{ac\lambda\pi r^2(r+s)^2 + lm\rho^2\pi}{a\rho^2\pi^2 r^2(r+s)^2} \\ = \frac{m\rho^2\pi r^2}{ac\lambda r^2(r+s)^2 + lm\rho^2}.$$

Neglecting the constant multiplier  $m\rho^2\pi$  and differentiating,

$$2ac\lambda r^3(r+s)^2 + 2rlm\rho^2 - 2ac\lambda r^3(r+s)^2 - 2ac\lambda r^4(r+s) = 0;$$

therefore

$$ac\lambda r^4 + ac\lambda sr^3 - lm\rho^2 = 0,$$

and

$$r^4 + sr^3 - \frac{lm\rho^2}{ac\lambda} = 0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

an equation having only two real roots, one positive and the other negative.

If  $s=0$  (that is, neglecting the silk covering), the equation would become

$$r = \sqrt[4]{\frac{lm\rho^2}{ac\lambda}}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In this case also  $r^4 = \frac{lm\rho^2}{ac\lambda}$ ; multiplying both sides by  $\pi$  and transposing, we get

$$\frac{ar^4\pi}{lm} = \frac{\rho^2\pi}{c\lambda},$$

therefore

$$\frac{lm}{ar^4\pi} = \frac{c\lambda}{\rho^2\pi}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The first member of equation (3) is identical with the expression given for the resistance of the coil if  $s=0$ , and the second member is the expression for the external resistance. The value of  $r$  in equation (2) is that of the radius of the wire, which, if we neglect the insulating covering, will give the maximum magnetic effect; and equation (3) shows that when wire of this size is used, the coil resistance will be equal to the external resistance.

Equation (1) is a biquadratic, and consequently rather trouble-



some to solve numerically. Provided  $s$  be small, we may get a very near approximation to the value of the positive root in the following way.

The value of  $r$  will, if  $s$  be small, not differ very widely from the value given in equation (2), namely

$$\sqrt[4]{\frac{lm\rho^2}{ac\lambda}},$$

which we will call  $b$ . Let

$$r = b + x,$$

equation (1) then becomes

$$(b+x)^4 + s(b+x)^3 - b^4 = 0.$$

Rejecting the second and all higher powers of  $x$ ,

$$(b+x)^4 = b^4 + 4b^3x,$$

$$s(b+x)^3 = +3sb^2x + sb^3$$

$$-b^4 = -b^4$$

$$(b+x)^4 + s(b+x)^3 - b^4 = (4b^3 + 3sb^2)x + sb^3 = 0$$

$$x = -\frac{sb^3}{4b^3 + 3sb^2} = -\frac{sb}{4b + 3s},$$

$$r = b + x = b - \frac{sb}{4b + 3s},$$

or

$$r = \sqrt[4]{\frac{lm\rho^2}{ac\lambda}} - \frac{s \sqrt[4]{\frac{lm\rho^2}{ac\lambda}}}{4 \sqrt[4]{\frac{lm\rho^2}{ac\lambda}} + 3s}, \quad \dots \quad (4)$$

an equation easily soluble by the aid of logarithms.

According to Dr. Matthiessen's experiments, the resistance of a wire of pure annealed copper, one-thousandth of an inch in diameter and one foot in length, at  $0^\circ$  Centigrade is 9.718 B.A. units. This being the case, if we give  $\rho$  the value of .0005,  $\lambda$  is very easily calculated for any given external resistance, being 1".23482 for each B.A. unit.

The following Table, giving the value of  $r$  as obtained by the numerical solution of equations (2) and (4), will show how nearly the two values approximate; and the difference between the resistance of the coil when wound with the wire of the calculated dimensions and the given external resistance will show how important it is to take the insulating covering into account when determining the size of the wire to use in winding the coil of an electromagnet or galvanometer to produce the best result in a given circuit.

Having obtained the value of  $r$  by means of either equation (2) or (4), it is easy to obtain the resistance of the galvanometer  $G$  in B.A. units by means of the following formula:—

$$\frac{12}{(\cdot0005)^2} : \frac{lm}{ar^2(r+s)^2c} :: 9\cdot718 : G;$$

therefore

$$G = \frac{\cdot000000202458 lm}{ar^2(r+s)^2c}. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Let  $m = 6$  square inches,

$l = 5''$ ,

$s = 0''\cdot000625^*$ ,

$\rho = 0''\cdot0005$ ,

$c = 1$ , or pure annealed copper at  $0^\circ$  Centigrade,

$a = 4$ .

1. External resistance, in B.A. units.	2. Value of $\lambda$ , in inches.	3. $r$ , calcula- ted by equa- tion (2).	4. $r$ , calcula- ted by equa- tion (4).	5. Resistance of galvano- meter, in B.A. units.
1000	1234\cdot82	''006091	''006097	907
2000	2469\cdot64	''005099	''005105	1782
3000	3704\cdot46	''004594	''004601	2641
10000	12348\cdot20	''003364	''003372	8432
100000	123482\cdot00	''001835	''001847	74345

A few words now with regard to the practical application of the theoretical results we have obtained may not perhaps be considered out of place.

It is at once evident that a very large alteration in resistance will be caused by even a very slight alteration in the size of the wire; it is therefore very important, when drawing wire for any given purpose, to measure the diameter very accurately. This may easily be done with a good screw-micrometer; but the instrument that I have found most useful for this purpose may be briefly described as follows (see fig. 4)†.

Two straight edges, each exactly  $10''$  long, are fastened together at one end, and separated at the other end by the insertion between them of a small piece of wire exactly  $0''\cdot1$  in diameter. One of the straight edges is graduated along its length into inches

\* This value of  $s$  was obtained by the actual measurement of the thickness of the silk on a sample of covered wire.

† The divisions and numbers on the right-hand side show also the diameters and numbers of the wires of the Birmingham wire-gauge, according to Culley's 'Hand-book of Telegraphy.'

and tenths, zero being where the edges meet. When it is necessary to measure the diameter of a wire, a piece is straightened and slid along the angular opening towards the smaller end until it will go no further. Its position, as shown on the graduated straight edge, will then give the diameter in parts of an inch, with sufficient accuracy for practical purposes, to three places of decimals\*. For instance, if the distance of the wire from the zero-point be  $2''\cdot5$ , the diameter will be  $0''\cdot025$ .

The thickness of the silk covering is easily measured by halving the difference between the diameter of covered and that of the uncovered wire.

Although in the Table I have calculated the radii of the wires to six places of decimals, yet this is chiefly to show how small the difference is between the result obtained by solution of the biquadratic equation (2) and that obtained by solution of the simple equation (4). It is, however, of course, practically quite unnecessary to carry the calculation beyond three places.

It must be remembered that the result obtained is the radius of the wire, and of course must be doubled to obtain the diameter.

XV. *Determination of the Absolute Horizontal Intensity of the Earth's Magnetism by measurement of a Current.* By F. KOHLRAUSCH †.

GAUSS'S method of measuring intensity, which together with the determination of declination forms a whole, is characterized by a classical simplicity of instrumental appliances, but requires for great accuracy the highest care of a practised observer. In the Göttingen *Nachrichten* of May 6, 1868, I communicated a determination of the three terrestrial magnetic elements by means of a new arrangement set up in the Göttingen Observatory, which, while it brought the determination of the inclination, by means of the terrestrial inductor, into the connexion between the measurement of the declination and of the intensity, at the same time, by improving the mechanism, essentially diminishes the labour and ensures greater accuracy. This arrangement shall be described in another place.

Yet every method of measuring the intensity which is restricted

\* The principle upon which this useful wire-gauge is founded was mentioned to me some years ago, I think by Mr. Britton of the Electric Telegraph Company; but I do not know who originated the idea, or whether any gauges of this form have been made elsewhere than in my workshops.

† Translated from Poggendorff's *Annalen* for September 1869, having been read before the Göttingen Royal Society, February 6, 1869.

to purely magnetic means, is in principle subject to a circumstance which occasions difficulties—that is, the unavoidable execution of the two necessary observations (of the oscillations and the deflections) *at different times*. Since the duration of the oscillations and the angle of deflection depend on the horizontal intensity of the terrestrial magnetism, the variations of the latter must be noted down as long as the absolute determination lasts. At the same time the variation-instruments must not be set up in the building in which the absolute measurement is being made. Lastly, the variations in the magnetism of the needle due to changes of temperature must be taken into account.

In an observatory in which continuous terrestrial magnetic observations are being made, these difficulties are far less perceptible than in the case in which the intensity is ascertained for purely physical purposes, as, for instance, for absolute voltaic measurements.

Just as the determination of the inclination by voltaic means which Weber introduced has facilitated this part of terrestrial magnetic measurements, so also voltaic determination greatly simplifies the determination of the intensity, inasmuch as all observations on which the earth's magnetism has any influence are simultaneously executed. A method shall now be described which simultaneously effects the absolute measurement of the magnetic intensity and of a voltaic current, and which is characterized by simplicity and convenience. Several years ago Weber tested this method, and, considering the then imperfect state of the instruments, obtained very satisfactory results, which encouraged a further development of the process. Hence last year all the instruments were constructed in a more perfect manner, and set up in the magnetic observatory at Göttingen. As a first instance of its application, I will communicate a measurement of intensity and of the current, from which it follows that the voltaic method is not only far more convenient than the purely magnetic one, but also seems not at all inferior in accuracy.

If one and the same voltaic current is passed through a tangent-compass, and through a solenoid with bifilar suspension (a bifilar galvanometer, compare *Resultate aus den Beob. des magnet. Vereins*, 1840, p. 93 *et seq.*) whose coils when at rest are parallel to the magnetic meridian, the deflection of the two instruments is proportional to the intensity,  $i$ ; that of the bifilar galvanometer is also proportional to the horizontal intensity of the earth's magnetism,  $T$ ; the deflection of the tangent-compass, however, is inversely as the latter magnitude. If, now, we know the diameter of the coils of the tangent-compass, as well as that of the surface contained by the coils of the bifilar galvanometer, and if, moreover, the statical directive force of the suspending-wires of



the latter be determined by measuring the moment of inertia and the time of oscillation, the two deflections will give the product  $iT$  and the ratio  $\frac{i}{T}$  in absolute measure, from which  $i$  and  $T$  can be determined separately. Just in this way, we know, the product and quotient of the magnetism of a bar into the earth's magnetism are ascertained in Gauss's method. Since the deflections of the tangent-compass and of the bifilar galvanometer are simultaneously observed, and the other measurements are independent of the earth's magnetism, the result of these determinations gives the horizontal intensity for the place and time of observation without any further reductions.

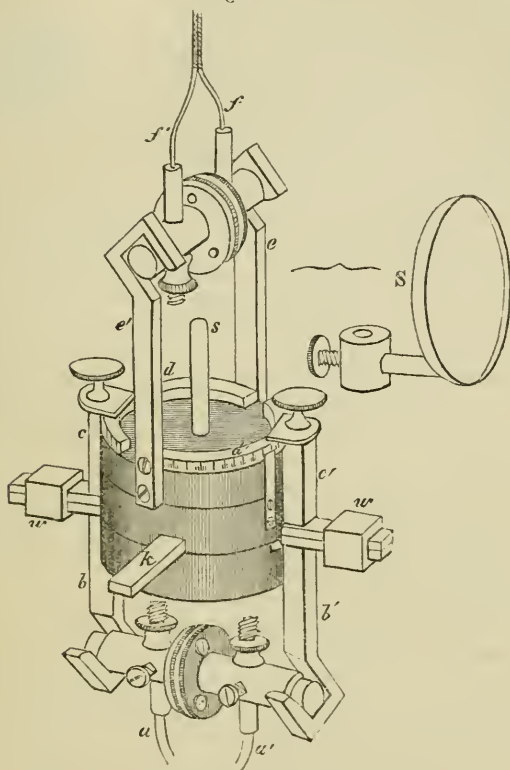
On a further comparison of the magnetic and voltaic methods a few other points appear in favour of the latter. First of all, the measurements of distance (here consisting of two measurements of the length of wires) only require to be made once, inasmuch as the coiling of the wires ensures the unalterability of the dimensions. Then, again, the difficult comparison of the magnetism of the needle in the meridian (in the oscillations) with the magnetism of the needle which is at right angles to the meridian (in the deflections) is quite dispensed with. In the third place, the observation of the deflections at two different distances is got rid of, inasmuch as the deflection of the needle is produced, not by a magnet in which the distribution of the magnetism is unknown, but by a current the distribution of which is most accurately known, and which has been brought into the simplest and most favourable form—that of a circle. Since there is only this single determination, the needle which is to be deflected may easily be so short that an approximate estimation of the position of the poles will be sufficient for the correction required. Finally, the magnitude of the forces and deflections are quite at our disposal, inasmuch as the current may have any desired strength.

Of the instruments, I shall have especially to devote to the *bifilar galvanometer* a short description elucidated by some figures. The suspended solenoid is a circular ring 67 centims. in diameter, formed of eighty-four coils of a covered copper wire 3.2 millims. in diameter. The ends  $a$  of this wire (fig. 1) are screwed and soldered to brass pins, which move with friction in a vertical direction in a horizontal axis, and are fastened above by means of removeable nuts. The axis is placed in the rectangular hooks of a suspension which is represented in fig. 1 half its natural size. All the parts represented in the figure as shaded are made of vulcanite; the others consist of gilt brass.

The suspension consists of three disks on the same rod,  $s$ , and turning against each other. In the middle one the rod is

fastened by a quadrangle, the lowest one is held by a nut (not shown in the drawing). The object of the lower rotation is to

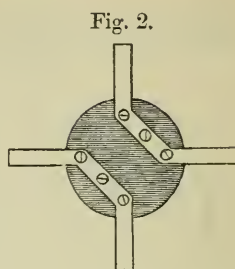
Fig. 1.



test with accuracy whether the plane of the coils coincides when at rest with the magnetic meridian; the object of the upper one is the production of this correct azimuth.

The rectangular hooks already mentioned are connected with the lower disk by the supports *b*. To the middle disk, on the contrary, the two supports *c* are fastened, which are seen in the figure as the continuations of *b*. In the arrangement represented, the middle and lower disks are firmly joined together, inasmuch as over two pairs of horizontal arms, the lower of which are screwed and soldered to *b* and the upper ones to *c*, tightly fitting hollow cubes, *w*, are passed. When the cubes are removed, by turning the lower disk through  $90^\circ$  the lower arms may be brought into the

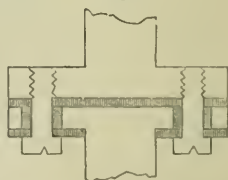
same relative position and connexion with two arms,  $k$ , of the middle disk, which form with the former an exact rectangular cross. In the drawing only one is visible. Each of these arms is the metallic continuation of an arm at right angles to it, as is seen in the accessory figure 2, which represents the middle disk seen from below. This disk has, moreover, an index which gives its position in reference to the upper one.



To the upper disk are fastened, first, the supports of the upper hook  $e$ , and, secondly, two metal sectors of a ring,  $d$ , each forming almost a semicircle. They are insulated from each other, but are each screwed to one of the supports,  $e$ , so as to form a conducting whole. Upon each of the half-rings there is a small metal plate, by means of which one of the supports,  $c$ , can be kept fast in any position in conducting communication with the ring. On the external surface the half-rings are provided with a scale divided into whole degrees, to which the previously mentioned index points.

The supports,  $e$ , have a hooked suspension above, with which the whole is suspended to an axis, which is fastened to the bifilar suspension (vide *infra*), as the lower one is to the solenoid. Both axes are exactly equal. Each consists of two parts, which (as fig. 3 shows, of the natural size) are firmly connected with, while insulated from, each other. In this section also the shaded part denotes vulcanite.

Fig. 3.



This being premised, the path of the electricity through the metallic parts of the suspension is easily understood. A current entering at  $f$  passes through  $e$ ,  $d$ ,  $c$ ,  $b$  to  $a$ , then through the solenoid, and through  $a'$ ,  $b'$ ,  $c'$ ,  $d'$ ,  $e'$  to  $f'$ .

The azimuth of the upper axis, and with it of the upper disk and of all parts connected with it, is shown by the suspending-wires. By turning the second disk the solenoid is placed in such a position that a current traversing it does not alter the position of rest. The plane of the coils of the solenoid is in this case at right angles to the magnetic meridian; hence it is only necessary to effect the cross position of the lower disk by turning it through  $90^\circ$ , to obtain the desired position of the plane of the coils in the meridian.

For the readings the rod  $s$  is provided with a mirror  $S$ , which for distinctness' sake is represented on the side of the drawing.

The *bifilar suspension* had to be different from the ordinary

one; for the conducting-wires, in order to ensure suspending-power and conductivity, had to be copper wires about 1 millim. in diameter. As the ordinary mode of suspension is attended with difficulties in the case of these strong wires, a solid cord was constructed of the two wires by plaiting them with silk in the manner represented in fig. 4, so that they were firmly connected with, while insulated from, each other. In constructing this cord, 170 centims. in length, to pass the shuttle containing the silk cord about 5000 times through the wires was a somewhat tedious operation. But the complete cord was remarkably firm; and while it was not possible by repeatedly stretching the two hard copper wires to get rid of the great curvatures in each, when thus joined they exhibited an almost faultlessly straight figure.

Fig. 4.



In order that the ends of the wire, where they are screwed and soldered into the pins of the upper axis, may not be liable to bend, a sheath is passed over the rod, the section of which is seen in fig. 5.

Fig. 5.



The insulation of the two wires, as well as of all the parts of the suspension, was tested with an extremely delicate galvanometer, and was found to be perfect.

Nothing need be said in reference to the *tangent-compass*, were it not that it had been constructed in a manner which is extremely simple and might be frequently imitated with advantage. It consists of a wooden disk of about 800 millims. diameter, upon two grooves in the rim of which a thick copper wire was wound in two coils. Suitable incisions in the disk give space for a small torsion-circle, the needle with a mirror (for the readings), and a powerful damper for the latter. The part of the wire projecting at the top is twisted to form a cord, as in the bifilar galvanometer, and, fastened to the ceiling, serves as a support for the whole instrument, which thus, held also laterally only by two clamps, has great solidity produced by the simplest means.

The connecting-wires of both instruments, together with the wires from the battery, are, everywhere, in order to exclude local action, placed in pairs near each other, and proceed to a *commutator* near the observing-telescope, by which the current in each branch of the conduction may be reversed.

The data which must be ascertained from the instruments once for all are the following:—

(1) The surface enclosed within the coils of the bifilar galvanometer. It is obtained from the length of the covered wire (177,318 millims.), and from the number (84) of coils formed by this length, taking into account that the coils, owing to gravity, have passed from the original circular shape into ellipses



the ratio of whose axes is equal to 1.056, and that the annular space occupied by the wire has a circular section of 36 millims. diameter.

Hence it follows that the desired surface

$$F = 29,774,300 \text{ square millims.}$$

(2) The semidiameter of the coils of the tangent-compass, also determined from the length of the coiled wire, is

$$r = 401,606 \text{ millims.}$$

(3) To ascertain by means of oscillations the directive force of the wires of the bifilar galvanometer, the moment of inertia of the latter must be known. It was found (determined by Gauss's method) to be

$$K = 432331 \cdot 10^6.$$

(4) The torsion-coefficient of the cocoon-thread to which the needle of the tangent-compass is suspended, compared with the directive force of the earth's magnetism, was found to be

$$\frac{\Theta}{mT} = 0.002528.$$

(5) The distance of one pole of the needle from the axis of rotation can be taken with sufficient accuracy as

$$l = 20 \text{ millims.}$$

As a specimen of a measurement of intensity and of current, two successive determinations made on the 23rd of October, 1868, are here given.

The time of oscillation of the solenoid was observed to be :—

$$\text{Before the deflections} \quad . \quad . \quad = 33.8082 \text{ seconds.}$$

$$\text{After the deflections} \quad . \quad . \quad = 33.7963 \quad ,,$$

Hence the mean  $t = 33.8023$  seconds.

From this is calculated the directive force of the suspending-wires,

$$D = K \frac{\pi^2}{l^2} = 37344 \cdot 10^5.$$

The current of a Grove's element (a resistance of three mercury units being inserted) was simultaneously passed through both instruments. A special series of experiments had shown that the directive force of the suspending-wires underwent no change even with a far stronger current. The bifilar galvanometer was deflected 429.78 millims., measured on a scale at a distance of 3960.8 millims. from the mirror; the needle of the tangent-compass 458.57 millims., on a scale at a distance of 3681.6

millims\*. The latter number must be increased to 459.02 millims. (that is, by 0.45 millim.), in consequence of the local action exercised by the bifilar galvanometer.

From this is calculated the angle of deflection of the bifilar,

$$\Phi = \frac{1}{2} \arctan \frac{429.78}{3960.8} = 3^\circ 5'.78;$$

and of the tangent-compass,

$$\phi = \frac{1}{2} \arctan \frac{459.02}{3681.6} = 3^\circ 33'.20.$$

If now  $T$  denotes the horizontal intensity of the terrestrial magnetism,  $i$  the intensity of the current, we get for the bifilar galvanometer the equation

$$D\Phi = iTF \cos \Phi \dagger,$$

from which

$$iT = 6.7881.$$

For the tangent-compass we have

$$\frac{4\pi i}{r} \left(1 + \frac{3}{4} \frac{l^2}{r^2}\right) \cos \phi = T \left(1 + \frac{\Theta}{mT}\right) \sin \phi,$$

from which

$$\frac{i}{T} = 1.9859.$$

From this is calculated

$$i = 3.6717, \quad T = 1.8488.$$

Interposing six mercury units, the deflections were 218.68 and 232.79 millims. respectively, whence

$$\Phi' = 1^\circ 34'.80,$$

$$\phi' = 1^\circ 48'.65,$$

and

$$i' = 1.8704, \quad T = 1.8499.$$

$T$  is here expressed in Gauss's absolute measure, and  $i$  in Weber's absolute magnetic units.

The concordance of the two values for  $T$  leaves nothing to be desired. Their mean value is 1.8493. Calculating from the secular formula the value of  $T$  for Göttingen, Oct. 23, 1868, we have 1.8452. There is thus found, from the magnetic observa-

\* These numbers are the means of the deflections represented by greater and smaller numbers.

† The directive force is the elasticity of the suspending-wires—which is corroborated by the fact that the time of oscillation seems independent of the amplitude. Hence the statical moment of rotation is proportional to the angle of deflection.

tions of the preceding year, the same result within 0.0022 of the whole amount. It would scarcely be possible to find a better example for showing the importance and significance of absolute measures than the agreement of the two numbers, which are not only based upon different observations, but even upon different natural laws.

The above determinations required for their execution less than an hour and a half. If, moreover, as it seems, the accuracy attainable is not inferior to that obtained by magnetic means, it will in future be a question whether the voltaic determination of *T* is not altogether to be preferred. It is undoubtedly so for voltaic purposes. Adding to the two galvanometers a Weber's terrestrial inductor, as in measuring inclinations, voltaic resistances may be determined in absolute measure; and, for most physical purposes for which no measurement of declination is needed, we have an adequate magnetic voltaic observatory for absolute measurements, in which, besides the needles used for measuring the current, all fixed magnets are avoided.

I will observe, in conclusion, that the construction of the instruments used for the above measurements, so far as it required a mechanical workshop, was executed by Dr. Meyerstein.

#### XVI. *Remarks on the Atomic Theory.*

By W. H. WALENN, F.C.S.\*

**D**R. WILLIAMSON has recently brought the atomic theory again under the consideration of chemists; and those who adopt his views of the theory of combining proportions and of multiple proportions (in short, his absolute interpretation of the atomic theory) must also be prepared to give their entire and unqualified assent to the following points:—

1. In regard to the indivisibility of so-called "atoms:—"To use the term "atom" in explaining a chemical combination is to assume the truth of a theory which has for its basis the real existence of atoms; and the word atom being derived from *ἄτομος*, a word signifying "indivisible," leaves the atomists no alternative but to suppose that, by the operation of parting or division, howsoever performed, upon a given mass of matter, a little mass or lump of the material taken at last remains, beyond which no division is possible.

2. In regard to the existence of physical atoms:—Since Dr. Williamson avows that in chemistry he acknowledges only one atomic theory, namely that of Dalton, and ignores the efforts of Lucretius (who was born B.C. 95) to develop the Epicurean

\* Communicated by the Author.

tenets respecting the formation of all things from atoms which existed from all eternity, as well as the moderate views of M. Laurent, he adheres in the most absolute manner to the belief of Dalton, that all bodies are composed of atoms which, however small, have a definite size and weight, and that by the varied combinations of these atoms all chemical substances may be produced; further, Dr. Williamson apparently believes this proposition to be incontrovertibly proved.

3. In regard to the consequences of points 1 and 2:—The atomists must be prepared to commit themselves to the consequences of their dogmas as well as to the dogmas themselves. Partly owing to the limited and isolated nature of the atomic theory, and partly to the inert condition imposed by it upon the mind, the individual characteristics of different chemical substances are not readily taken into account. The condition of matter as a force-carrier, or as a means of manifesting certain forces, is entirely ignored by the atomic theory, and is thereby placed beyond the reach of the chemical mind; the only force that is recognized is that of gravity, the exponent of which is weight. The atomic theory comprises only the idea of little lumps of various kinds of matter with their relative weights and combining numbers; it then totally and entirely stops.

The unity and harmony of *true* science is a principle which the mind of man has always either tacitly admitted or explicitly believed. It is upon this basis, and from a physico-mathematical point of view, that the above-mentioned points will now be very briefly considered.

In reference to the first point, it is unfortunate that in many parts of his paper Dr. Williamson writes of atoms and atomic weights as if the existence of atoms was proved, whereas his avowed object is to prove the existence of those atoms. There can be no doubt that Dr. Williamson has served science well and has done that which will greatly promote the progress of scientific truth; he has put forward his ideas in a most definite manner, and in a way which affords a faithful representation of the views of a large class of chemists; but many will fail to find the subject logically treated at his hands, his results being isolated from the facts from which he deduces them, and the reasoning employed being discontinuous and not by any means leading to the end he is desirous of establishing. Any one of the various points of evidence that is brought to bear on the atomic theory is sufficient to cast a grave doubt upon its truth; and the whole testimony fully warrants chemists in using the prudent and moderate language of Dr. Roscoe rather than such positive assertions:—"The law of multiple proportions, being founded on experimental facts, stands as a fixed bulwark of the science, which



must ever remain true; whereas the atomic theory, by which we now explain this great law, may possibly in time give place to one more perfectly suited to the explanation of new facts."

In mathematical science, as, for instance, in the Differential and Integral Calculus, the greatest advantage has been derived from reasoning upon quantities that, in relation to other quantities, diminish without limit; but this diminution, arising as it does from a specific operation or repetition of processes upon a given quantity, never implies annihilation, as Dr. Williamson evidently infers when, in order to evolve his results, he is obliged to represent particles to his mind by supposing the operation of division of a sensible mass into parts (by successive steps) to stop at a certain point yet undetermined. Division of a given mass of matter, say, by successively halving it, will never annihilate it, but will always leave sufficient of it to reason upon, although it may by constant repetition of the process become far too small to be recognizable by the balance; moreover, however small it may be, it will possess the intrinsic properties of the given mass. Since this view of the subject never supposes the annihilation of matter, it is always competent for the mind to realize the building up of a mass of matter by the multiplication of the particles to which it has been reduced, possibly for the convenience of theoretical investigation. When, again, the measure of comparison is weight, it serves no purpose to suppose that matter consists of a collection of individual atoms; for these are not practically weighable, more especially as there is no proof to the eye, the touch, or other sense of the existence of such atoms; the practical way in this case is evidently to take a definite and arbitrary unit of weight, such as a gramme of hydrogen, for instance, to start with.

In reference to the second point, if  $a_1, a_2, a_3, \dots a_n$  represent certain definite weights of the different kinds of matter represented by  $A_1, A_2, A_3, \dots A_n$ , related to each other in the same way as the so-called "atomic" weights of the said different kinds of matter (so that  $a_1$ , instead of being one indivisible unit of hydrogen, shall mean one gramme of hydrogen,  $a_2$  sixteen grammes of oxygen, &c.), the proportions of these definite weights in relation to each other and in relation to the law of multiple proportions will be the same as if any other definite weights (however small) were taken. It follows that the existence of physical atoms is not only not a necessary consequence of the facts that have been observed respecting the combining proportions of various substances, or the multiple proportions of pairs of substances, but is quite immaterial to the question at issue. The assumption of a unit independent of the laws of nature is not new in science, but is a necessary means of orderly arrangement and of establishing re-

lative numerical values,—*e. g.* Hofmann's crith, the British Association unit of electric resistance, horse-power as a unit of motive power, and Sir Benjamin Brodie's  $\alpha$  (which is the symbol of that portion of hydrogen that occupies the volume of 1000 cubic centimetres at 0°C. and a pressure of 760 millimetres of mercury). In an analogous way, a gramme of hydrogen, sixteen grammes of oxygen, fourteen grammes of nitrogen, &c. may be conveniently taken to realize and apply both theoretically and practically the combining proportions of various chemical substances.

In reference to the third point, the circumscribed ideas that spring from the atomic theory led to the belief, more or less established according to the mind that receives the theory, that atoms were spherical and separated by space from each other. The difficulties of this position in the present state of science are so great that even Dr. Williamson disclaims any specific statement about the shape of atoms, thereby removing still further from the region of fact the atoms that are dealt with in his paper; for to accept the existence of a certain portion of matter without even knowing its form is certainly not impossible, but nevertheless adding one more to the points of evidence that may be adduced against its existence at all. If the train of reasoning respecting the inability to annihilate matter by continued division be adopted, it follows that however small a particle be taken to reason upon, if it be a crystal, for instance, it must have the same shape as the mass from which it is derived. However small a piece of chloride of sodium be taken, as crystallized at common temperatures, it will be a cube; if crystallization takes place at a temperature below freezing-point, prismatic crystals containing water of crystallization will be obtained. By carrying out this line of argument in other cases of crystallization, it will be clearly seen that the smallest particles that be can conceived at a given temperature are of the same shape as that of the mass whence they are derived at the same temperature. This method of analyzing the question dispenses entirely with the inert little lumps that the atomic theory presupposes, and may enable crystallization, for instance, to be followed out in all its bearings without the encumbering element of weight. The action of other forces upon matter may possibly be investigated thus, without the consideration of weight: these forces doubtless manifest themselves by vibrations of a greater or less extent and of various configurations; among them are the mechanical vibrations which produce sound, the luminous vibrations which give light, and the chemical vibrations which form the actinic force.

If another argument were required to show that these very atoms, which some minds consider so tangible, have only an existence in the minds of atomists and not a real existence, the fact

that some chemists suppose one kind of atom to exist for mechanical science and another kind for chemical science would supply the want. The chemical and mechanical atoms are supposed in reality to be identical; that is, identity is predicable of two things that are not the same.

The writer believes that a false step has been unwittingly taken by some chemists in using the word atom without considering that the very foundation upon which they were building their mental conceptions of the laws of combination was simply an hypothesis; repetition, combined with an illusive appearance of reality and certainty, doubtless contributed to the hold which physical atoms have upon some master minds. Dalton unquestionably required the notion of physical atoms to define multiple proportions to himself; but at the present day they are a hindrance, and must be classified with the crystalline spheres of Ptolemy, the electric fluid of the early electricians, and the phlogiston of Stahl.

74 Brecknock Road, London, N.,  
December 1869.

## XVII. *Fundamental Principles of Molecular Physics.*

*By* Professor W. A. NORTON.

[Concluded from vol. xxxviii. p. 214.]

SINCE the publication of the continuation of my reply to Professor Bayma in the September Number of the *Philosophical Magazine*, my attention has been diverted to other more urgent matters, which have left me little time or inclination to continue the discussion. This I have little occasion to regret, as, upon a full consideration of the case, I have come to the conclusion that the proper point has been reached for bringing the controversy between Professor Bayma and myself to a close. His objections to my theoretical views have been answered as fully as the case seems to demand, and he has replied at length to my criticisms of his own views. We now stand upon the same ground. Whatever temptation there may be to expose the futility of many of the arguments urged in support of his positions, and call attention to the misconceptions into which he has fallen, I am willing to rest the case here in the hands of the intelligent and candid reader—only entering a disclaimer against the position taken by my learned and agile opponent, at the close of his reply, that we differ only on “a few points of secondary importance,” and agree, in the main, in our fundamental views. The reader who has had the patience to follow us in this protracted discussion, will not fail to perceive wherein we differ and

wherein we agree, and that the controversy, so far from having been about non-essentials, has been for the most part about radical differences of doctrine or of scientific method. What actual scientific importance this diversity of fundamental views may have, the future must decide. In bringing our amicable controversy to a close, I must not withhold my acknowledgment of the courtesy and general fairness manifested by my opponent, both in the attacks he has made upon my positions, and in the maintenance of his own.

### XVIII. *Memoir on the Expansion of Gases.*

By M. V. REGNAULT\*.

I HAVE collected in this memoir the numerous experiments I have made during the last twenty years, to determine the losses of heat which a gas undergoes when it expands under the very various conditions in which this phenomenon is met with in nature and in our laboratories. I shall briefly indicate the idea which has led me to undertake these researches; the reader will thus more readily understand the successive phases through which they have passed.

When in 1842 I commenced my experiments to determine the calorific capacity of gases, I thought I had found a certain and easy method of determining with the same apparatus and without any *petitio principii*:—

1. The specific heat of a gas under constant pressure and changing volume;
2. Its specific heat under constant volume, the pressure varying so as to leave the volume constant.

In the *Mémoires de l'Académie*, vol. xxxi. p. 58, I have described the experiments I have made with the view of determining the specific heat of a great number of vapours and gases under a constant pressure but with variation of volume; I have no need to revert to them. I will merely say that in these experiments the heated gas traversed a calorimeter with a constant velocity, retaining virtually the same pressure. The heat which it gave to the calorimeter served for calculating the calorific capacity of the gas between the limits of temperature which the gas had attained. In this mode of working there is only a single, extremely small correction—that due to the circumstance that the hot gas arrives at the calorimeter with a far greater molecular velocity than that with which it emerges.

To obtain with the same apparatus the specific heat under a constant volume, I made the heated gas enter the calorimeter

\* Translated from the *Comptes Rendus*, October 11, 1869.



with such an excess of pressure that, when the gas expanded to the surrounding atmospheric pressure, its volume should remain constant notwithstanding the change of temperature.

With the ideas I then had as to the structure of gases, and which were at that time generally admitted by physicists, I thought I had thus realized the conditions under which the specific heat of a gas under constant volume can be directly obtained.

I intended to make these new experiments when those which gave the specific heats under constant pressure had been terminated. But these latter occupied me during several years, and I decided on the 17th of July, 1848, to deposit in the Academy of Sciences a sealed packet which contains a complete description of my method for determining successively, with the same apparatus, the specific heat of a gas under constant pressure and that of this gas under constant volume (*Comptes Rendus*, vol. xxvii. p. 77).

Nevertheless the first experiment which I thus made, the 22nd of October, 1849, gave me a far different result from that which I expected: I found that *the gas undergoing expansion in the calorimeter indicated the same calorific capacity as that which it gave me when it traversed the calorimeter without undergoing expansion.*

To render this fact more striking, I greatly increased the pressure under which the heated gas reached the capillary orifice. I then observed that the heat given up by the hot gas when it undergoes a great expansion in the calorimeter, is even a little greater than that which is given by the same gas while traversing this calorimeter and at the same time retaining its elastic force.

Thus, *under the conditions in which my experiments were made*, the specific heat of gas under constant volume is virtually equal to the specific heat of this gas under constant pressure. I announced this fact to the Academy on the 18th of April, 1853 (compare *Comptes Rendus de l'Académie des Sciences*, vol. xxxvi. p. 680).

I had also to conclude from my experiments that, *if the compressed air enters the calorimeter at the same temperature as the calorimeter, the expansion should take place therein without causing it to undergo a sensible change of temperature.*

This in fact is what I proved by a great number of experiments.

In short, my researches have shown me that, *when a gas expands under the conditions of my experiments (that is to say, when it leaves the calorimetrical apparatus with the whole of the motion which the expansion has given to it), the calorific phenomenon is far different from that which obtains for the same gas when it is contained in a state of rest in a cylinder and when its volume is made to vary by displacing a piston.*

At the present time the mechanical theory of heat quite ex-

plains these differences: it shows that it is not sufficient to consider the initial and final densities of the gas, but that account must be taken of the changes produced in the motion of translation of the gas, and even in its molecular motions.

It is easy to define *theoretically* a certain number of conditions in which a gas may undergo the same change of density and experience different variations of temperature. But it is far more difficult, and often even impossible, to realize these conditions by experiment, *retaining the simplicity of the theoretical enunciation*. Most frequently secondary phenomena are produced which cannot even now be clearly defined, and of which it is still more difficult to measure or calculate the effects.

I shall distinguish in this memoir two kinds of expansion; for I have had to employ for each of them a special method:—

1. Simple expansion, which I shall call *statical expansion*; it is that which a gas *at rest* undergoes when *by the aid of external work* the space it occupies is increased, the gas resuming the *state of rest* after the expansion. There is the *same mass* of gas in a state of rest in the calorimetric reservoir at the commencement and at the end of the experiment.

2. The expansion which I shall call *dynamical*; this is when a gas either at rest or in motion expands in traversing an orifice, and then enters or escapes from the calorimetric apparatus with all its acquired velocity. The mass of the gas contained in the calorimetric reservoir *is not constant* during the experiment: one portion enters or escapes, with a variable velocity determined by the varying excess of pressure.

I shall commence with the dynamical expansion, although it is the most complex; but it is that which I first studied, as it frequently occurred in all my researches on gases. The numerous experiments which refer to it may be divided into two parts.

The first comprises those in which a gas compressed and in motion reaches the calorimeter and emerges from it with its original motion modified by the effect of the expansion. The calorimeter contains then at the commencement and at the end of the experiment the same very small quantity; its temperature is modified *solely* by the calorific effects that the gas undergoes which traverses it.

The second part refers to experiments in which the total quantity of gas is contained in a calorimetric reservoir either at the commencement or at the end of the experiment. I shall here distinguish two cases.

*First case.*—The compressed gas is in a state of rest in the calorimetric reservoir. It is allowed to escape by opening a more or less capillary orifice; it escapes into the atmosphere with the varying velocity produced by its successive expansion.

*Second case.*—The calorimetrical reservoir contains gas at rest under the pressure of the external atmosphere. The gas compressed in a large reservoir escapes by a capillary orifice and passes at a known temperature into the calorimetrical reservoir, the gas of which it successively compresses until the elastic force has become the same in the two reservoirs.

*Dynamical Expansion.—First Part.*

- I. *Heat absorbed by the expansion of a compressed gas when it traverses a small orifice in a thin plate.* (Experiments of 1850 to 1854.)

The gas is compressed in a large reservoir; it traverses a long serpentine which keeps it at a constant temperature as far as its capillary orifice, through which it expands and becomes in equilibrium with the external pressure. The change of temperature which it thus undergoes is determined by the calorimetrical method. I have discriminated two cases.

*First case.*—The compressed gas reaches the capillary orifice with a temperature but little different from that of the calorimeter. In this case—

Atmospheric air undergoes a diminution of temperature, which amounts to  $0^{\circ}.3176$  for a diminution in elastic force of 1 metre of mercury.

In the case of hydrogen, the variation in temperature is imperceptible.

Lastly, for carbonic acid, the lowering of temperature is more considerable than for atmospheric air; for it reaches  $1^{\circ}.64$  for a diminution in pressure of 1 metre of mercury.

These effects are due to several causes acting simultaneously.

1. Gases which deviate from Mariotte's law in the direction ascertained for air and carbonic acid undergo an *excess of expansion* which gives rise to a lowering of temperature proportional to this excess; as hydrogen deviates from this law in the opposite direction, it probably disengages a small quantity of heat.

2. The gaseous molecules are at rest in the large reservoir; on passing through the capillary orifice, not only do they expand and drive back the external atmosphere, consuming the work expended by the tranquil gas in the reservoir, but they assume successively a great velocity of translation, and therefore a *vis viva* which is produced by their own heat, and which determines a lowering of temperature which is frequently greater than that due to the first cause.

3. In proportion as the gas expands in the calorimeter the velocity of translation of the molecules diminishes, and there is a disengagement of heat which partially compensates for the cooling due to the second cause; but there cannot be exact compen-

sation, because the gas always emerges from the calorimeter with a great velocity.

For atmospheric air, and especially for carbonic acid, the resultant is a notable cooling.

For hydrogen, the three causes, acting in contrary directions, neutralize each other, especially owing to the small atomic weight of the gas.

*Second case.*—The compressed gas reaches the calorimeter with a temperature much higher than that of the calorimeter, so that there is not merely expansion but also cooling of the gas.

This second case is more complicated than the first, because it comprehends the calorific effects produced by changes of molecular velocity, the modifications produced in the constitution of the gas by change of pressure, and those, moreover, which arise from change of temperature. I have been led to consider a special calorific capacity which comprehends the whole of these effects.

For air, this calorific capacity with expansion is appreciably greater than the calorific capacity of air under constant pressure.

For hydrogen it is the same.

Lastly, for carbonic acid it is considerably smaller than the calorific capacity under constant pressure at high temperatures, and much greater at very low temperatures.

## II. *Experiments to determine the heat which atmospheric air absorbs when it expands from a high pressure to the pressure of the atmosphere, not suddenly as in the preceding chapter, but successively.* (Experiments of 1853.)

In the preceding experiments the air compressed in the reservoir expands completely on emerging from a single orifice in the interior of the calorimeter; the expanded gas circulates then in the superposed boxes of the calorimeter, and acquires the temperature of the water of the calorimeter before emerging into the atmosphere. I have endeavoured in the experiments I am about to describe to effect this expansion step by step at several periods in the interior of the calorimeter, so as to ascertain whether this circumstance exerts an influence.

The special calorimeter which I used for these experiments, and which I shall call *calorimeter for successive expansions*, consists of a series of brass tubes A, B, C, D, . . . , 8 millims. in the clear, connected with each other by tubulures. These tubes, twelve in number, are vertical, and form a circular crown in the interior of the calorimetric vessel.

The reservoir containing the compressed gas communicates with a long worm arranged in a bath of water. The end of this



worm fits somewhat tightly in the tubulure of the first tube A; the junction is hermetically cemented.

When the stopcock of the large reservoir is opened, the gas circulates in the worm, retaining almost the same pressure as in the reservoir, but acquiring the temperature of the water of the bath. This pressure is in like manner transmitted to the first tube A of the calorimeter; expansion only commences as the gas emerges from the capillary orifice of the first tube A. A second expansion takes place as the gas passes through the second capillary orifice, so that the elastic force of the gas is less in the tube C than in the tube B. On emerging from each of the vertical tubes, there is an expansion, until the gas reaches the twelfth and last tube, where it is virtually under the pressure of the atmosphere.

The capillary apertures which the gas thus successively traverses have gradually larger diameters as they become more distant from the orifice, in order that the relative value of the expansion may be nearly the same for each orifice, notwithstanding the successive variation which the pressure experiences in each vertical tube.

The experiment is otherwise made in exactly the same manner as when the calorimeter was used with superposed boxes and a single expansion.

Experiments made by means of the calorimeter with successive expansions gave the following values of  $y_1$  (that is to say, the lowering of temperature experienced by the gas for a diminution in pressure of 1 metre of mercury):—

Mean excess of pressure.	Weight of air escaped in a minute.	$f_1$ .	$y_1$ .
millimetres.	grammes.	metre.	
4780	34.5	1.000	0.3590
6080	44.0	"	0.3290
7421	50.3	"	0.3580
6607	8.9	"	0.3781
6314	13.6	"	0.3621
5763	37.2	"	0.3426
		Mean of $y_1 = -0.3548$	

The last three experiments were made under almost the same pressure, but with very different velocities of outflow. They show that the value of  $y_1$  is greater the less the velocity of the expanded gas on its emergence from the calorimeter. I give the reason for this in my memoir.

In order that the results of various experiments may be per-

fectly comparable, the velocity of the gas in emerging from the calorimeter should be exactly the same. It would be particularly interesting to arrange the apparatus and conduct the experiments so that the velocities of transport of the expanded gas were the same, on emerging from the calorimeter, as those which they possess when they arrive with their initial pressure in the first tube of the calorimeter. For this purpose the sections should be inversely as the densities of the gases. I have not, however, attempted to realize this condition in the experiments I have just described.

It would also be of great interest to know the value of  $y_1$  when the gas emerges from the calorimeter with an infinitely small velocity; this would be *the limit of y*. It is clear that this cannot be attained merely by a calorimetrical method; for the conditions cannot be realized in which the gas finally assumes the state of rest and remains constantly at the same temperature as the calorimeter. It could only be obtained by determining very accurately the temperature of the gas on its emerging from the calorimeter, and the velocity of the current at the place where the bulb of the thermometer which gives this temperature is placed. In my memoir many experiments are given in which I have endeavoured to determine the temperature of the gas on its emergence; but this determination always presents uncertainties.

### III. *Calorific effects produced by air which expands in traversing more or less long capillary tubes.* (Experiments of 1854.)

In my first mode of working, the air suddenly expands on emerging from a single capillary orifice perforated in a thin plate. In the second the expansion was made successively on passing through twelve orifices in thin plates placed at equal distances from each other. In the third method, which I am about to describe, I wished the expansion to take place in an absolutely continuous manner, the air traversing a very long capillary tube. The air reached the entrance of this tube under a pressure very little different from that which it had in the large reservoir, and on escaping from this tube it met a pressure very little different from that of the atmosphere. I was especially desirous of ascertaining thus whether the friction of the air on a great length of tube-wall modified to any perceptible extent the calorific effects produced by the simple expansion of the gas.

The large reservoir in which the air is compressed is hermetically connected with the end of a long copper worm placed in a bath filled with water at the surrounding temperature. The other end of this worm is hermetically cemented to the commencement of the worm placed in the calorimeter.

The worm of the calorimeter consists of two copper tubes

wound on the same cylinder; each of these tubes is  $1\frac{1}{2}$  metre in length, and their internal section is 6 millims. Between these two tubes is intercalated a silver capillary tube, the internal calibre and length of which are varied. This tube is coiled in a spiral on the same cylinder as the portions of the copper spiral between which it is intercalated, and it is hermetically cemented by means of tin-solder. The water of the calorimeter, and that of the bath (which gives to the air a known temperature before it reaches the worm of the calorimeter), are kept in a perfectly uniform state of agitation. The temperatures are indicated by thermometers, each Centigrade degree of which has twenty divisions, and which have been accurately compared at the same temperatures as those they are exposed to in the experiments.

When the stopcock of the reservoir with compressed air is opened, the air traverses the worm of the bath without sensible change of pressure, but assuming the temperature of the water, and passes without any change of pressure into the first part of the copper worm of the calorimeter. Expansion commences at the entrance into the narrow silver worm; and on emerging from this capillary tube the air flows through the second part of the copper worm, where it resumes the pressure of the atmosphere, and emerges after having assumed exactly the temperature of the water of the calorimeter.

In four series of experiments the length of the capillary tube varied from 0.09 metre to 1.79 metre; the quantities of gas which emerged in a minute varied from 2.2 grms. to 13.5 grms., while the values of  $\gamma$  remained almost exactly the same. Yet, from the results of the fourth series, it must be admitted that they increase with the velocity of the outflow—that is to say, inversely as the length of the tube. This fact may be attributed to heat disengaged by the friction of the gas along the sides; but it is far more probable that it arises from the fact that the gas retains then a greater velocity on emerging from the calorimeter, and that it thus carries off a greater quantity of heat in the state of *vis viva*.

In three new series the lengths of the capillary tube varied from 0.07 metre to 1.16 metre; the quantities of gas which emerged per minute changed from 12 to 46 grms.; but the values of  $\gamma$  only underwent very small alterations: they increase with the velocity of the outflow more regularly than in the group of the first four series. We arrive therefore at the same conclusion.

In other experiments I suppressed the two pieces of wider copper tube between which the capillary silver tube was intercalated, so that the air emerged directly from the capillary tube into the atmosphere.

The values of  $\gamma$  then became for the same pressure much greater than in the preceding series, in which the expansion of the gas took place entirely in the calorimeter. Thus the cooling is far greater when the silver capillary tube debouches directly out of the calorimeter into the external air than when the gas on passing from the capillary tube can expand completely in the boxes of the calorimeter, and then traverse them with a small velocity before escaping. This obviously arises from the circumstance that in the first case the gaseous molecules constantly increase in velocity until their emergence from the calorimeter, while in the second case the gas undergoes, on the contrary, a material loss of *vis viva* in the boxes of the calorimeter, to which it thus abandons a considerable quantity of heat, which diminishes by so much the cooling observed.

These latter experiments show that the cooling is produced during the circulation and by the expansion of the gas in the silver capillary tube, while *the end of the expansion on the outside, with the modifications which are the consequence of it, rather gives rise to a disengagement of heat, owing to the destruction of vis viva.* I wished to establish this fact more directly by experiment, and I had recourse to an apparatus consisting of two calorimeters, A and B. The first calorimeter, A, small in capacity, contains the silver capillary tube rolled spirally; the first end of this tube fits in the long copper worm immersed in the large water-bath, and which brings the compressed air from the reservoir; the other end of the silver tube fits in the lower box of the second calorimeter B, which is like those I have used to determine the calorific capacity of gases under the pressure of the atmosphere. This second calorimeter terminates in a tube through which the gas is liberated. This latter tube fits in a larger tubulure, in which is kept a very delicate thermometer, and which by a lateral tubulure conducts the gas to a meter.

Experiments made with this new apparatus led to the following conclusions:—

1. The cooling effect of the expansion is produced almost entirely in the silver capillary tube. Owing to the great conductivity of silver and the smallness of the section of the tube, the gas emerges from the capillary tube sensibly at the same temperature as the water of the first calorimeter, but with a far greater molecular velocity than it had on entering.

2. A new expansion takes place in the superposed boxes of the second calorimeter; this expansion is much weaker. The small lowering of temperature which may be thus produced is more than compensated by the heat which the gas abandons in losing the greater part of its *vis viva*; so that the total effect consists in a *considerable heating* of this calorimeter.



3. The expansion is almost complete on emerging from the second calorimeter; but the gas still retains its velocity. If it passes into a space of larger section, the velocity of its molecular transport diminishes; a disengagement of heat ensues, which produces an appreciable elevation of the temperature of the gas.

4. To obtain the true quantity of heat taken from the surrounding medium by the gas which expands under the conditions of the experiment, and without change of temperature, the gas should emerge from the second calorimeter destitute of *vis viva* (a condition which in practice it is impossible to realize), or with a *vis viva* equal to that which it had on entering.

In continuation of these long researches I was led to construct a single calorimeter in which are performed all the expansions which successively take place in the two connected calorimeters. I have definitively adopted this arrangement for atmospheric air and for all other gases, because it seemed to combine most guarantees for exactitude. I have endeavoured to define as exactly as possible the conditions in which the gas undergoes its successive expansions.

Numerous experiments have given for  $y_1$  a greater value than that I obtained in the expansion of air through orifices in a thin plate—that is to say,  $y_1 = -0^{\circ}\cdot377$  for a variation in pressure of 1 metre of mercury. This is the value I definitively assume for atmospheric air when the expansion takes place under the clearly defined conditions of the experiment.

I have previously spoken of the experiments I made to determine the heat absorbed by carbonic acid in motion when it expands without altering its temperature. This quantity of heat is far greater for carbonic acid than for air; it appears moreover to increase sensibly with the pressure. In these experiments the absolute pressure never exceeded 9·9 metres of mercury.

I was desirous of working with the new apparatus under far higher pressures, especially under those which gaseous carbonic acid retains in the reservoir of Thilorier's apparatus, in which it has been liquefied. I conducted the experiment as follows.

The reservoir of Thilorier's apparatus, containing about 4 kilogs. of liquid carbonic acid, is kept in a large vessel of water at the surrounding temperature, which maintains it at a virtually constant temperature. I assume that the metallic mass of this reservoir constantly restores the heat absorbed by the volatilization of the liquid acid. A very strong copper tube connects the worm of the large bath with the atmosphere of gaseous carbonic acid of the reservoir. The calorimeter is arranged exactly as it was for the experiments on atmospheric air; it is furnished with the same thermometers. A continuous and perfectly regular agitation mixes the layers of water of the calorimeter and those of the vessel which contains the large worm.

The apparatus being arranged and the stirring having been continued for a quarter of an hour, I make observations during the first five minutes, during which the calorimeter is only subject to external disturbing causes; an assistant then suddenly opens the stopcock of Thilorier's reservoir. The gaseous carbonic acid bursts into the worm of the vessel, where it produces the pressure of the reservoir, then passes through the capillary worm of silver, where it expands, and enters the superposed boxes of the calorimeter with a slight excess of pressure over the external air.

A water manometer, communicating by a tubulure with the upper gas-box of the calorimeter, only indicates an excess of pressure of 50 millims., which corresponds to 3.7 millims. of mercury. Thus the gas only retains a very slight excess of pressure on emerging from the calorimeter.

The flow is permitted to last four minutes; this time is sufficient for the passage of about 120 litres of carbonic acid. The stopcock of the reservoir is then suddenly closed, the observation of the thermometers continued minute by minute until the tenth minute. Finally, the variations of the thermometer of the calorimeter are followed during the last five minutes, when it is no longer subjected to any but external disturbing causes.

The greatest care must be devoted to the construction and preservation of the apparatus; for it must keep a gas under a pressure of about fifty-five atmospheres without the least loss.

The elastic force of the carbonic acid which reaches the calorimeter is calculated from the temperature  $t$  of the water surrounding the Thilorier reservoir, and by the aid of the formula which I have given (*Mémoires de l'Académie*, vol. xxxi. p. 130). I assume that this elastic force is maintained from the reservoir until the arrival of the gas at the entrance of the silver capillary worm. It is probable that it is really somewhat less; but the difference must be very small, for the section of the silver capillary is but a tenth that of the large worm of the vessel.

The volume of the carbonic acid used in each experiment is indicated by the gas-meter.

Combining the results thus obtained for carbonic acid under high pressure with those I previously obtained under lower pressures, I have constructed the following Table:—

Excess of pressure producing the outflow.	Cooling experienced by the gas.	Value of $y_1$ .
millims.		
3354	— 5.434	— 1.619
7764	— 12.731	— 1.6396
38591	— 81.960	— 2.1238

It will be seen that the cooling increases more rapidly than the excess of pressure which produces the outflow.

The considerable cooling which carbonic acid undergoes by its expansion when it retains its acquired *vis viva*, explains several phenomena which have been observed on carbonic acid liquefied in Thilorier's apparatus. Thus, when the liquid acid escapes from the reservoir through a narrow orifice, and is received in a small open receiver, a great part of the liquid congeals to a snowy mass which fills the whole of the receiver. This effect is ordinarily attributed to the heat absorbed by carbonic acid when it assumes the gaseous state; this heat would be imparted to it by the carbonic acid which remains liquid, the temperature of which thus sinks below its freezing-point. If this explanation were correct, the solid acid should present the appearance of a continuous mass applied against the sides of the receiver, and not as a snowy mass which completely fills it. The evaporation of the liquid certainly gives a gas whose temperature is very low; but the subsequent expansion of this gas greatly depresses its temperature again; so that it is really the gaseous carbonic acid formed which congeals, and not that which remains liquid.

When the gaseous acid which forms the atmosphere above the liquid acid is allowed to escape from the reservoir by a capillary orifice, it is observed that the orifice is frequently closed by small crystals of solid acid. To the cold produced by the expansion is due this momentary congelation.

In fine, my experiments with capillary silver tubes prove that when a gas flows, even with considerable velocity, along very extended sides, there is no appreciable disengagement of heat which could be attributed to the friction of the gaseous molecules against these sides.

This conclusion is opposed to the view generally held; and many facts may be cited which seem to contradict it. I will mention the most important.

A projectile which traverses the air with great velocity becomes much heated. This fact is attributed to the heat disengaged by the friction of the projectile against the molecules of the air which it traverses.

Meteorites fall through our atmosphere with extreme velocity; they become heated to incandescence, and melt, either entirely or merely on their surface. This fact is attributed to the heat disengaged by friction against the gaseous molecules.

I think that in both cases the heat comes from another cause, and is merely due to the heat disengaged by the compression of the air.

When a body in motion traverses the air with a greater velo-

city than that of sound, the effects of the elasticity of the air are annulled, and the compression produced by the moving body has not time to reach the adjacent layers before they in their turn are compressed by the body. In consequence of this inertia the air becomes compressed as it would be in a pneumatic syringe. A great part of the heat which is the result of this compression passes into the moving body, the temperature of which it raises. Moreover the moving body is not affected by the expansion of the air producing cold; for this expansion does not take place till after it has passed. Thus, in my opinion, the moving body always, proceeding with the same velocity, collects by the compression of the air the heat which it disengages, and is not affected by the cooling produced by the subsequent expansion of the layers of air which it has just traversed.

It is evident moreover that the compression of the air will be more energetic the greater the velocity of the moving body; the temperature of the moving body will rise then successively until it is equal to that assumed by a layer of air which instantaneously undergoes the same compression in the pneumatic syringe. Thus is well explained the very high temperature which a meteorite assumes when traversing the air with a far greater velocity than that of the propagation of sound.

A heating of the same kind, but less, would be produced in a moving body which traversed the air with a lower velocity than that of sound. In this case also the moving body would be more influenced by the heat disengaged by compression than it would be by the absorption of heat by expansion. The two effects would virtually compensate each other when the velocity was very small.

In my opinion there is no heat disengaged by the friction of two bodies, except when the molecules of at least one of them are not quite free—that is to say, when they are under the influence of some force of aggregation. From our observations this absolute liberty would only be met with in the imponderable fluids, such as the æther which transmits the luminous vibrations. It is not perfect in our gases; and from this fact alone the motion of a gas against a solid must disengage a certain quantity of heat, which results solely from the transformation into heat of the loss of *vis viva* undergone by the molecules in overcoming their internal resistances. My experiments prove that this quantity of heat is so small for atmospheric air that it escapes our means of observation.

Liquids have always more or less viscosity, which proves that their particles have not perfect mobility. The passage of a liquid through a tube must therefore disengage by friction an appreciable quantity of heat; and this quantity must vary with differ-



ent liquids. I have several times made experiments in this direction, but they are not yet complete enough for publication.

In solids, lastly, especially in those which are very hard, the molecules have very little mobility; the friction of these bodies against each other must, then, produce considerable disengagement of heat, even when there is no disaggregation. In all cases the disengaged heat arises from the transformation of a perceptible external motion into a molecular vibratory motion about the position of equilibrium, which is only manifested by its calorific effects; so that the disengagement of heat by friction always corresponds to a loss of *vis viva*.

Mathematical theory supposes that bodies possess perfect elasticity; this is implicitly assumed in establishing the primordial equations. It is supposed, moreover, that the motion cannot change its nature nor be communicated to other media. Lastly, no account is taken of the materiality of bodies; for no allowance is made for the modifications which gravity introduces into molecular motions. In the case of the æther its gravity need not be taken into account; but the modification is recognized which the elasticity of the æther undergoes in transparent material substances, because it is necessary to admit this in order to explain refraction.

Hence the laws deduced from the mathematical theory of elasticity should only be capable of absolute verification in the case of media which possess perfect elasticity; and we only observe this in the case of the æther which transmits luminous vibrations. Experiment shows us that the velocity of propagation in the æther is excessive as compared with what we observe in material bodies. May we not conclude thence that in a medium of absolutely perfect elasticity the velocity of propagation would be *infinite*? According to this, the æther alone approaches far towards this perfection; but there still remains to it some materiality, which leaves it under the influence of material bodies, and in virtue of which it offers an appreciable resistance to the motion of the stars which traverse it.

However this may be, I think that the degree of elastic perfection of a body may be appreciated by the velocity with which it transmits a disturbance. The velocity of propagation is greater in solids than in liquids, and greater in liquids than in gases. Hence gases should deviate from the mathematical laws of elasticity more than solids and than liquids, which in fact is what experiment proves.

*Determination of the quantities of heat absorbed by a gas in motion which undergoes sudden or successive variations of density, by observing the temperatures it possesses in different parts of its course.*

If it were possible to determine simultaneously the tempera-

ture and elastic force in the various parts of a current of gas when it undergoes sudden or successive modifications of its density, we should have a simple means of determining the quantities of heat which are lost or gained in the various parts of its course. My memoir contains a great number of experiments made in this direction.

I have employed in succession mercurial thermometers, air-thermometers, and thermoelectric elements. The expansion sometimes took place through an orifice in a thin plate, sometimes through a capillary tube, and sometimes through porous plates. It is impossible to give an account of these experiments within the limits to which I must confine this extract.

A large reservoir contains air under a pressure of 8 metres of mercury which is maintained virtually constant. It is enclosed, as usual, in a large vessel of water, the temperature of which remains unchanged during an experiment. The reservoir communicates with a worm placed in the same vessel; its length is 12 metres, and internal section 20 millims.

The end of the worm just projects from the side of the large vessel, and is closed by a metal plate in which is perforated a small orifice. A tube of thin brass, the length of which can be varied, fits, with some friction, on the projecting part of the worm.

The temperature of the air after its escape through the capillary orifice is determined by a mercurial thermometer, the bulb of which can be brought more or less near the orifice.

When the bulb of the small thermometer is at about 10 millims. from the capillary orifice, it marks  $1^{\circ}8$  less than the thermometer of the large vessel. If it be gradually removed from the orifice, the temperature rises rather rapidly, and the difference of temperature is only some tenths of a degree when the bulb of the small thermometer approaches the orifice of the large terminal tube.

If from the distance of 10 millims., at which there was a difference in temperature of  $1^{\circ}8$ , the bulb of the small thermometer is gradually brought nearer the orifice, the difference of temperature, instead of increasing, diminishes very briskly; it even changes in sign when the bulb of the thermometer is brought almost into contact with the sides of the orifice.

Thus in this experiment the small thermometer shows the greatest cooling when its bulb is 10 millims. from the orifice. The cooling diminishes as the bulb is removed from the capillary orifice, because the velocity of the gas diminishes; it also diminishes, and that very rapidly, when the distance is made less than 10 millims. And the cooling changes into an apparent heating when the bulb almost touches the orifice.

These results are not in discordance with those observed by Messrs. Joule and Thomson under analogous conditions. But I explain them otherwise, and I draw from my experiments very different conclusions from those which the English physicists have deduced from theirs.

Thomson and Joule's experiments on the passage of air through an orifice in a thin plate gave far more considerable diminutions of temperature than those I obtained under analogous circumstances. This may in a measure arise from the fact that the velocity of outflow was greater in their experiments, but especially, I think, from the fact that in their experiments the air is directly compressed by the pump in the long tube of uniform section, where it circulates rapidly as far as the small orifice through which it diffuses into the atmosphere. But I think in this case the temperature is far from being uniform in the various parts of the tube.

The air arrives greatly heated by the working of the pump: I admit that it loses this great excess of heat during its long passage through the double worm; but the layers of air near the orifice furnish directly the work which drives the gas outwards, losing thus a certain quantity of heat which they have not time to take from the fresh layers which arrive to replace them. In short, I think we cannot assume for a *natural* gas in motion the principles which are propounded *theoretically* for the ideal gas, even when they would be almost true for the natural gas in a state of rest. As to the *vis viva* which the molecules assume in expanding, inasmuch as its creation only takes place at the passage of the orifice, it certainly only takes place at the expense of the heat of the *emerging* gas, the temperature of which should thus be much lowered.

In previous memoirs I have frequently dwelt on the slowness with which air enclosed in a metal reservoir, surrounded by a large mass of water, reacquires the temperature of this water when it has been cooled by the expansion it experiences when a portion escapes. When this reservoir is cylindrical, and its diameter is only 20 centims., from ten to fifteen minutes elapse before the open air-manometer which communicates with this reservoir resumes the stationary condition.

Messrs. Joule and Thomson speak of a considerable disengagement of heat which they have observed when air strikes against an obstacle opposed to its emergence, especially when it grazes the bulb of a thermometer which is kept in the conical space of a gutta-percha funnel, thus greatly diminishing the section which remains free for the passage of the gas, &c. I have never succeeded in observing such a phenomenon; and the fact appears to me contradictory to the experiments I have made with the

worms formed of silver capillary tubes, where I could never observe any heat disengaged by friction.

In my experiments on the flow of air through an orifice in a thin plate, I observed that the thermometer indicated an increase of temperature when its bulb was placed almost in contact with the capillary orifice; but I attributed this fact to an alteration in shape of the bulb arising from the unequal pressures which the current of air impressed upon it. The following experiment proves that this is the true explanation.

I emptied the mercury of my thermometer and replaced it by alcohol. The instrument thus became far more sensitive to variations in temperature, owing to the greater expansibility of alcohol, but the variations produced by external pressure remain the same. Now the thermometer which had thus been modified never indicated a higher temperature than that of the bath, even when I brought the reservoir into contact with the capillary orifice. These experiments showed, moreover, that we should not use mercurial thermometers to determine temperatures in rapid currents of gas; and I have had recourse to air-thermometers, on the action of which the compression of the envelope exerts no appreciable influence.

Now the first fact I have thus observed is that, in all positions of the thermometer placed in expanded air, this thermometer indicates a lower temperature than that marked by the thermometer placed in compressed air. The difference in temperature is greater the nearer the bulb is to the capillary orifice; the maximum is attained when the bulb is pressed against this orifice, the surfaces not being sufficiently even to produce complete closing.

This latter fact is completely opposed to the elevations of temperature noted by Messrs. Thomson and Joule.

In a second series of experiments the bulb of the air-thermometer placed in the expanded air had a larger diameter, so that it entered the tube with slight friction, and remained there. There was then a very narrow interval between this bulb and the tube which surrounded it. I hoped thus to realize the conditions in which the English physicists observed a great rise of temperature.

Now it is precisely under these circumstances that I obtained the most cold; it is then that the difference of temperature between the two thermometers varies least with the distance of the bulb from the orifice. The explanation of these facts appears very simple: in the present arrangement the air undergoes two successive expansions—the first on emerging from the capillary orifice, the second in the very narrow annular space of great diameter which exists between the bulb of the thermometer and



the tube surrounding it. Owing to this second expansion and the increase of *vis viva*, there is a great diminution in the temperature of the air before and around the thermometer, of the expanded air; and that explains the excess of cold indicated by this thermometer.

In my experiments the first expansion was greater than the second; but this might have been easily reversed by increasing the section of the capillary orifice.

The experiments I have made with thermoelectric elements have led to the same results; they have, moreover, enabled me to follow far more easily the variations in temperature which the gaseous current undergoes. I shall only mention a few.

The expansion takes place in a capillary glass tube 4 centims. in length, terminated by a glass tube 30 millims. in internal diameter, in which is arranged one of the elements, which can thus be brought more or less near the capillary orifice; and the soldering may even be placed in this orifice.

1. The thermoelectrical solderings exhibit differences of temperature which are gradually smaller in proportion as one of the solderings is removed from the orifice of the capillary tube.

2. The greatest difference of temperature is observed when this soldering is quite in the interior of the capillary tube, the section of which it considerably diminishes. It is evident, moreover, that, if the section were diminished beyond a certain limit, the difference of temperature would, on the contrary, be diminished; for if the orifice were entirely closed, the two solderings would be at the same temperature.

To realize still more closely the conditions in which Messrs. Thomson and Joule observed a great increase of temperature, I arranged the following experiment.

In the tubulure of the large worm I fit a thick disk of gutta percha, perforated in the centre by a small aperture surmounted by a hollow cone. The thermoelectric element is sealed in the axis of a gutta-percha cylinder terminated by a projecting cone, which exactly fits in the hollow cone of the thick disk. The soldering of the element is just level with the apex of the cone. It is needless to say that the disk of gutta percha is supported by a metal plate with a large aperture, which prevents it from being deformed and even torn by the pressure of the compressed air.

We here again see the lowering of temperature rapidly diminish as the soldering is removed from the aperture of the disk. It rapidly increases, on the contrary, when the cone which envelops the soldering is pressed into its position.

The greatest degree of cooling is obtained with a certain approximation of the two cones. When they are brought nearer, the cooling diminishes; it also diminishes when they are moved

further apart. But in neither case is the difference in temperature inverted, as would be the case if there were a considerable disengagement of heat produced by the friction of the air against the sides.

I shall not speak here of experiments I have made on the passage of air through porous plates; in order to make them intelligible, it would be necessary to describe apparatus, give numerical results, and enter into details inconsistent with the limited extent of this extract.

### *Dynamical Expansion.—Second Part.*

In the first part of this memoir I endeavoured to determine the calorific effects produced by a gas which reaches a calorimetric apparatus under a great pressure, which there expands to the external atmospherical pressure, and emerges with the temperature of this calorimeter. Thus the gas subjected to the experiment only traverses the calorimeter, in the conditions I have mentioned, and no part of the gas remains there. The calorific effect is mainly produced by variations of the *vis viva*.

In the second part the gas subjected to experiment is originally contained in the calorimeter in a state of rest, or arrives successively in the calorimeter under the influence of an excess of pressure, to remain there at rest until the end of the experiment. We thus work alternately by compression and expansion.

In this mode of working, the statical expansion plays the principal part in the effect produced; but the dynamical expansion intervenes, in conditions resembling those realized in the experiments of the first part of this memoir. Clément and Désormes's method, with the modifications it has recently undergone on the part of several experimenters, comes within the same category. The essential difference in my mode of working consists in the fact that the quantities of heat are determined by the calorimetric method, while my predecessors sought the differences in temperature.

These researches occupy the greater part of my memoir; I have repeated them several times, altering the arrangement of the apparatus, which here exerts a very great influence. It is impossible for me to give an idea by a short extract and without the aid of figures.

### *Statical Expansion.*

In the third part of my memoir I have endeavoured to realize experimentally the conditions of the statical expansion of gases, measuring simply the changes of temperature produced. I am again obliged to refer to my complete memoir, which will soon appear; for the mere statement of my method would involve lengthy explanations.

XIX. *Proceedings of Learned Societies.*

## ROYAL SOCIETY.

[Continued from p. 73.]

June 17, 1869.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read :—

“Researches on Vanadium.”—Part II. By Henry E. Roscoe, B.A., Ph.D., F.R.S.

*On the Chlorides of Vanadium and Metallic Vanadium.*

In the first part of these researches (‘Bakerian Lecture,’ Phil. Trans. 1868, pt. i.) the author stated that the chlorides of vanadium, and probably also the metal itself, could be prepared from the mononitride, the only compound of vanadium not containing oxygen then known. The process for obtaining the mononitride described in the last communication was that adopted by Berzelius for preparing the substance which he conceived to be metal, but which in reality is mononitride. This method consists in the action of ammonia on the oxytrichloride; but it cannot be employed for the preparation of large quantities of nitride, owing to the violence of the action and consequent loss of material. The author, seeking for a more economical method, found that if the ammonium metavanadate ( $\text{NH}_4\text{VO}_3$ ) be heated for a sufficiently long time at a white heat in a current of dry ammonia, pure vanadium mononitride remains behind. Analysis of a sample thus prepared gave 79.6 per cent. of vanadium and 20.2 per cent. of nitrogen, theory requiring 78.6 and 21.4 per cent. respectively. The mononitride may likewise be directly prepared by igniting vanadium trioxide ( $\text{V}_2\text{O}_5$ ) in a current of ammonia at a white heat in a platinum tube, and also by subjecting the dichloride to the same treatment.

*The Chlorides of Vanadium.*—Three chlorides of vanadium have been prepared, viz. :—

Vanadium tetrachloride . . . .	$\text{VCl}_4$
Vanadium trichloride . . . . .	$\text{VCl}_3$
Vanadium dichloride . . . . .	$\text{VCl}_2$

1. *Vanadium Tetrachloride*,  $\text{VCl}_4$ , molec. wt. = 193.3, V.d., = 96.6 ( $\text{H} = 1$ ).—This chloride is formed as a dark reddish brown volatile liquid, when metallic vanadium or the mononitride is burnt in excess of chlorine. The first method adopted for the preparation of this chloride was to pass dry chlorine over the mononitride heated to redness; the whole of the nitride volatilizes and a reddish-brown liquid comes over. In one operation 44 grammes of the crude tetrachloride was thus prepared; the liquid is purified by distillation first in a current of chlorine and then in a stream of carbonic acid gas. On fractionating, the liquid was found to boil at  $15.4^\circ\text{C}$ . (corrected)

under 760<sup>mm</sup> of mercury. The second method depends upon a fact already noticed in the preceding communication, that the oxytrichloride ( $\text{VOCl}_3$ ), prepared, according to the directions of Berzelius, by passing dry chlorine over a mixture of the trioxide and charcoal, possesses a port-wine colour instead of the canary-yellow tint of the pure substance. This dark colour is due to the formation of the tetrachloride of vanadium; and if the vapours of the oxytrichloride, together with excess of dry chlorine, be passed several times over a column of red-hot charcoal the whole of the oxygen of the oxychloride can be removed, and at last perfectly pure tetrachloride, boiling constantly at  $154^\circ$ , is obtained. This reaction, it will be remembered, served first to demonstrate the existence of oxygen in the oxytrichloride. In each distillation of the tetrachloride a peachblossom-coloured solid residue remained in the bulbs; this substance is vanadium trichloride, and it slowly burns away in excess of chlorine when heated, forming tetrachloride.

The composition of the tetrachloride was established by six well-agreeing analyses, made from several different preparations. The mean result is:—

	Calculated.	Found.
V = 51.3 . . . . .	26.54 . . . . .	26.87
$\text{Cl}_4 = 142.0$ . . . . .	73.46 . . . . .	73.02
<hr/>	<hr/>	<hr/>
193.3	100.00	99.89

Owing to the facility with which the tetrachloride splits up into trichloride and chlorine, a solid residue was left in the vapour-density bulb, and the density of the vapour (at  $219^\circ$ ) was found by Dumas's method to be 99.06 (or 6.86) instead of 96.6 (or 6.69). By volatilizing the liquid in a small bulb, and allowing the vapour to pass into a large bulb already heated above the boiling-point of the liquid, this deposition of trichloride was avoided, and the density was found to be 96.6 or 6.69 at  $205^\circ$ , and 93.3 or 6.48 at  $215^\circ$ , the last determination indicating that a partial decomposition into  $\text{VCl}_3$  and  $\text{Cl}$  had occurred. The specific gravity of the liquid tetrachloride at  $0^\circ$  is 1.8584; it does not solidify at  $-18^\circ$ , nor does it at this or any higher temperature undergo change of properties on treatment with chlorine. It not only undergoes decomposition on boiling, but at the ordinary atmospheric temperatures it splits up into  $\text{VCl}_3$  and  $\text{Cl}$ . Tubes in which the liquid tetrachloride had been sealed up have burst by the pressure of the evolved chlorine. Thrown into water, the tetrachloride is at once decomposed, yielding a blue solution identical in colour with the liquid obtained by the action of sulphurous or sulphydric acid on vanadic acid in solution, and containing a *vanadous* salt, derived from the tetroxide  $\text{V}_2\text{O}_4$ . In order to prove that a vanadous salt is formed when the tetrachloride is thrown into water, the solution thus obtained was oxidized to vanadic acid by a standard permanganate solution. The calculated percentage of oxygen thus needed according to the formula  $2\text{VCl}_4 + \text{O} + 4\text{H}_2\text{O} = \text{V}_2\text{O}_5 + 8\text{HCl}$



is 4.14; the percentage of oxygen found by experiment was 4.11.

The solution of the tetrachloride in water does not bleach; but if the vapour be led into water a liquid is obtained which bleaches litmus. Vanadium tetrachloride acts violently on dry alcohol and ether, forming deep-coloured liquids. The author is engaged upon the examination of this reaction.

Bromine and vanadium tetrachloride, sealed up and heated together, do not combine; on the contrary, trichloride is deposited. Hence it is clear that vanadium does not readily form a pentad compound with the chlorous elements.

2. *Vanadium Trichloride*,  $\text{VCl}_3 = 157.8$ .—The trichloride is a solid body, crystallizing in splendid peachblossom-coloured shining tables, closely resembling in appearance the crystal of chromium sesquichloride. It is non-volatile in hydrogen, and, when heated in the air, it decomposes, glowing with absorption of oxygen, and forming the pentoxide. Heated in hydrogen the trichloride first loses one atom of chlorine, forming the dichloride ( $\text{VCl}_2$ ), and afterwards, on exposure to a higher temperature, loses all its chlorine, leaving metallic vanadium as a grey lustrous powder. The trichloride is extremely hygroscopic, deliquescing on exposure to air to a brown liquid. The trichloride is best prepared by the quick decomposition of the tetrachloride at its boiling-point, or by its slow decomposition at the ordinary temperature of the air. The crystalline powder obtained by either of these methods only requires freeing from adhering tetrachloride by drying in carbon dioxide at  $160^\circ$  in order to yield good analytical results.

	Calculated.	Mean of 4 analyses.
V = 51.3 . . . . .	32.5 . . . . .	32.57
$\text{Cl}_3 = 106.5$ . . . . .	67.5 . . . . .	67.42
<hr/> 157.8	<hr/> 100.0	<hr/> 99.99

The trichloride thrown into water does not at once dissolve; but as soon as the crystals get moistened, a brown solution is formed, which becomes green on addition of a drop of hydrochloric acid, and contains a *hypovanadic* salt in solution. This green tint is identical with that got by reducing a solution of vanadic acid in presence of magnesium. According to the equation  $2\text{VCl}_3 + \text{O}_2 + 3\text{H}_2\text{O} = \text{V}_2\text{O}_5 + 6\text{HCl}$ , the solution of the trichloride requires 10.14 per cent. of oxygen to bring it up to vanadic acid, whilst analysis showed that 10.1 per cent. was necessary. The specific gravity of the trichloride at  $18^\circ$  is 3.00.

3. *Vanadium Dichloride*,  $\text{VCl}_2 = 122.3$ .—The dichloride is a solid crystallizing in fine bright apple-green micaceous plates. It is prepared by passing the vapour of vanadium tetrachloride mixed with hydrogen through a glass tube heated to dull redness. If the heat be pushed further a blackish crystalline powder, consisting of a mixture of lower chloride and metal, is obtained. The dichloride, when strongly heated in hydrogen, loses all its chlorine, leaving

vanadium in the metallic state in grey crystalline grains. Analysis gave :—

	Calculated.	Mean of 2 analyses.
V = 51.3 . . . . .	41.95 . . . . .	42.16
Cl <sub>2</sub> = 71.0 . . . . .	58.05 . . . . .	57.88
<hr/> 122.3	<hr/> 100.00	<hr/> 100.04

Vanadium dichloride is extremely hygroscopic; when thrown into water a violet-coloured solution is formed, identical in tint with the liquid containing a *hypovanadous* salt obtained by reducing vanadic acid in solution in presence of zinc- or sodium-amalgam; and like this latter liquid, the solution of dichloride in water bleaches strongly by reduction.

Oxidized by permanganate this liquid required 18.78 per cent. of oxygen (on the dichloride taken) to bring it up to vanadic acid, whereas the equation  $2\text{VCl}_2 + \text{O}_3 + 2\text{H}_2\text{O} = \text{V}_2\text{O}_5 + 4\text{HCl}$  requires 19.6 per cent. The specific gravity of vanadium dichloride at 18° is 3.23.

*Metallic Vanadium*, V=51.3.—Although from what we now know of the characters of vanadium it appeared unlikely that any compound containing oxygen would yield the metal by direct reduction, the author has repeated the experiments of other chemists on this subject, but without success. There is no doubt that the metal cannot be obtained by any of the processes described in the books. The only methods which promised possible results were :—

1. The reduction of a vanadium chloride (free from oxygen) in hydrogen gas, either with or without sodium.
2. The reduction of the mononitride at a white heat in hydrogen.

The first of these methods has proved to be successful, whilst the second does not appear to yield metal, inasmuch as the nitride exposed for  $3\frac{1}{2}$  hours in a platinum tube to the action of hydrogen at a white heat, lost only 8 per cent., whereas it must lose 21.4 per cent. on conversion into metal.

Notwithstanding the apparent simplicity of the method, the author has found it exceedingly difficult to obtain the metal perfectly free from oxygen. This arises from the fact that, whilst vanadium is quite stable at the ordinary temperature, it absorbs oxygen with the greatest avidity at a red heat, and that therefore every trace of air and moisture must be excluded during the reduction. Another difficulty consists in the preparation of the solid chlorides in large quantity and free from oxygen or moisture, as also in the length of time needed to reduce these chlorides in hydrogen, during which time an avoidable diffusion occurs and traces of oxygen enter the tube. Again, the reduction can only be effected in platinum boats placed in a porcelain tube, as the metal acts violently on glass and porcelain, and tubes of platinum are porous at a red heat.

A description of the apparatus employed is then given, the main points being to guard against diffusion, and to introduce the powdered

dichloride into the platinum boat in such a way that it shall not for an instant be exposed to moist air. After all precautions are taken, the tube is heated to redness, torrents of hydrochloric acid come off, and the evolution of this gas continues for from 40 to 80 hours, according to the quantity of dichloride taken. After the evolution of any trace of hydrochloric acid has ceased to be perceptible, the tube is allowed to cool, and the boat is found to contain a light whitish grey-coloured powder, perfectly free from chlorine.

Metallic vanadium thus prepared examined under the microscope reflects light powerfully, and is seen to consist of a brilliant shining crystalline metallic mass possessing a bright silver-white lustre. Vanadium does not oxidize or even tarnish in the air at the ordinary temperature; nor does it absorb oxygen when heated in the air to  $100^{\circ}$ . It does not decompose water even at  $100^{\circ}$ , and may be moistened with water and dried *in vacuo* without gaining weight. The metal is not fusible or volatile at a bright red heat in hydrogen; the powdered metal thrown into a flame burns with the most brilliant scintillations. Heated quickly in oxygen it burns vividly, forming the pentoxide; but slowly ignited in air it first glows to form a brown oxide (possibly  $V_2O$ ), and then again absorbs oxygen and glows, with formation of the black trioxide and blue tetroxide, till it at last attains its maximum degree of oxidation. The specific gravity of metallic vanadium at  $15^{\circ}$  is 5.5. It is not soluble in either hot or cold hydrochloric acid; strong sulphuric acid dissolves it on heating, giving a yellow solution; hydrofluoric acid dissolves it slowly with evolution of hydrogen; nitric acid of all strengths acts violently on the metal, evolving red nitrous fumes and yielding a blue solution; fused with sodium hydroxide the metal dissolves with evolution of hydrogen, a vanadate being formed.

One sample yielded on oxidation a percentage increase of 77.94, whereas that calculated from metal to pentoxide is 77.98. Another preparation gave a percentage increase of 70.8, showing the presence of a small quantity of oxide. On treatment in a current of chlorine, metallic vanadium burns and forms the reddish black tetrachloride; heated in a current of pure nitrogen, the mononitride is formed.

The properties of the compounds of vanadium with silicon and platinum are then described in the memoir.

“On the Continuity of the Gaseous and Liquid States of Matter.—Bakerian Lecture.” By Thomas Andrews, M.D., F.R.S. &c.

In 1863 the author announced, in a communication which Dr. Miller had the kindness to publish in the third edition of his ‘Chemical Physics,’ that on partially liquefying carbonic acid by pressure, and gradually raising at the same time the temperature to about  $88^{\circ}$  Fahr., the surface of demarcation between the liquid and gas became fainter, lost its curvature, and at last disappeared, the tube being then filled with a fluid which, from its optical and other properties, appeared to be perfectly homogeneous. The present paper contains the results of an investigation of this subject, which has occupied the author for several years. The temperature at which carbonic

acid ceases to liquefy by pressure he designates the critical point, and he finds it to be  $30^{\circ}92$  C. Although liquefaction does not occur at temperatures a little above this point, a very great change of density is produced by slight alterations of pressure, and the flickering movements also described in 1863 come conspicuously into view. In this communication, the combined effects of heat and pressure upon carbonic acid at temperatures varying from  $13^{\circ}$  C. to  $48^{\circ}$  C., and at pressures ranging from 48 to 109 atmospheres, are fully examined.

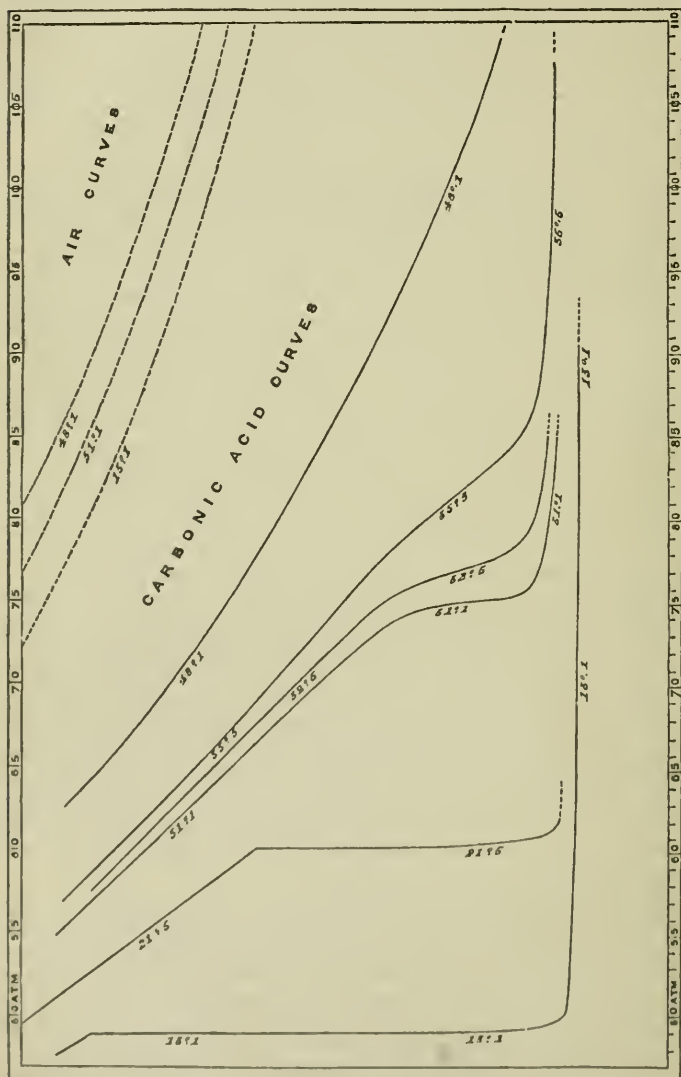
At  $13^{\circ}1$  C., and under a pressure, as indicated approximately by the air manometer, of 48.89 atmospheres, carbonic acid, now just on the point of liquefying, is reduced to  $\frac{1}{80.9}$  of the volume it occupied under one atmosphere. A slight increase of pressure, amounting to  $\frac{1}{20}$  of an atmosphere, which has to be applied to condense the first half of the liquid, is shown to arise from the presence of a trace of air ( $\frac{1}{1000}$  part) in the carbonic acid. After liquefaction, the volume of the carbonic acid, already reduced to about  $\frac{1}{470}$  of its original volume, continues to diminish as the pressure augments, and at a much greater rate than in the case of ordinary liquids. Similar results were obtained at the temperature of  $21^{\circ}5$ . A third series of experiments was made at  $31^{\circ}1$ , or  $0^{\circ}2$  above the critical point. In this case the volume of the carbonic acid diminished steadily with the pressure, till about 74 atmospheres were attained. After this, a rapid but not (as in the case of liquefaction) abrupt fall occurred, and the volume was diminished to one-half by an additional pressure of less than two atmospheres. Under a pressure of 75.4 atmospheres, the carbonic acid was reduced to  $\frac{1}{311}$  of its original volume under one atmosphere. Beyond this point it yielded very slowly to pressure. During the stage of rapid contraction there was no evidence at any time of liquefaction having occurred, or of two conditions of matter being present in the tube. Two other series of experiments were made, one at  $32^{\circ}5$ , the other at  $35^{\circ}5$ , with the same general results, except that the rapid fall became less marked as the temperature was higher. The experiments at  $35^{\circ}5$  were carried as far as 107 atmospheres, at which pressure the volume of carbonic acid was almost the same as that which it should have occupied if it had been derived directly from liquid carbonic acid, according to the law of the expansion of that body for heat.

The last series of experiments was made at  $48^{\circ}1$ , and extended from 62.6 to 109.4 atmospheres of pressure. The results are very interesting, inasmuch as the rapid fall exhibited at lower temperatures has almost, if not altogether, disappeared, and the curve representing the changes of volume approximates closely to that of a gas following the law of Mariotte. The diminution of volume is at the same time much greater than if that law held good.

The results just described are represented in a graphical form in the figure (p. 152). Equal volumes of air and carbonic acid, measured at  $0^{\circ}$  C. and 760 millimetres, when compressed at the temperatures marked on each curve, undergo the changes of volume indicated by the form of the curve. The figures at the top and bottom indi-



cate the approximate pressures in atmospheres; the volumes of the gas and air are measured upwards from the dotted horizontal line.



The author has exposed carbonic acid, without making precise measurements, to higher pressures than any of those mentioned, and has made it pass, without breach of continuity, from what is uni-

versally regarded as the gaseous to what is, in like manner, universally regarded as the liquid state. As a direct result of his experiments, he concludes that the gaseous and liquid states are only widely separated forms of the same condition of matter, and may be made to pass into one another by a series of gradations so gentle that the passage shall nowhere present any interruption or breach of continuity. From carbonic acid as a perfect gas, to carbonic acid as a perfect liquid, the transition may be accomplished by a continuous process, and the gas and liquid are only distant stages of a long series of continuous physical changes. Under certain conditions of temperature and pressure, carbonic acid finds itself, it is true, in a state of instability, and suddenly passes, without change of pressure or temperature, but with the evolution of heat, to the condition which, by the continuous process, can only be reached by a long and circuitous route.

The author discusses the question as to what is the condition or state of carbonic acid when it passes, at temperatures above  $31^{\circ}$ , from the ordinary gaseous state down to the volume of the liquid without giving any evidence during the process of the occurrence of liquefaction, and arrives at the conclusion that the answer to this question is to be found in the intimate relations which subsist between the gaseous and liquid states of matter. In the abrupt change which occurs when the gases are compressed to a certain volume at temperatures below the critical point, molecular forces are brought into play which produce a sudden change of volume; and during this process it is easy to distinguish, by optical characters, the carbonic acid which has collapsed from that which has not changed its volume. But when the same change is effected by the continuous process, the carbonic acid passes through conditions which lie between the ordinary gaseous and ordinary liquid states, and which we have no valid grounds for referring to the one state rather than to the other.

Nitrous oxide, hydrochloric acid, ammonia, sulphuric ether, sulphuret of carbon, all exhibited critical points when exposed under pressure to the required temperatures.

The author proposes for the present arbitrary distinction between vapours and gases, to confine the term vapour to gaseous bodies at temperatures below their critical points, and which therefore can be liquefied by pressure, so that gas and liquid may exist in the same vessel in presence of one another.

The possible continuity of the liquid and solid states is referred to as a problem of far greater difficulty than that which forms the subject of this communication, and as one which cannot be resolved without careful investigation.

XX. *Intelligence and Miscellaneous Articles.*

ON THE SIMULTANEOUS OCCURRENCE OF A SOLUBLE LEAD-SALT AND FREE SULPHURIC ACID IN SHERRY WINE; WITH OBSERVATIONS ON THE SOLVENT ACTION OF ALCOHOLIC SALINE SOLUTIONS UPON SULPHATE OF LEAD. BY PROFESSOR F. H. STORER.

SEVERAL years since, I was called upon by a wine-merchant of this city to examine a sample of pale sherry taken from a cask which had been returned to him, on the certificate of a chemist that the wine contained lead. The sample in question was perfectly transparent and clear. There was nothing in the appearance or taste of the wine to indicate the sophistication to which it had really been subjected.

On submitting this sherry to chemical analysis, I found not only that it held in solution a considerable proportion of lead, but also a decided trace of free sulphuric acid, besides an abundance of the same acid combined with some alkaline base. When a portion of the wine was evaporated in contact with slips of paper, the latter soon became crumbly and friable.

Regarded merely from the chemical point of view, without reference to its manifest bearing upon questions of hygiene and jurisprudence, the simultaneous occurrence of a lead-salt and of free sulphuric acid in alcoholic solution is a fact sufficiently important to merit close attention. Unfortunately the small sample of wine given me was completely exhausted in the severe confirmatory tests by which the results above mentioned were controlled, and I have had no opportunity to determine the precise manner in which the lead was held in solution in that particular case. Several conjectures as to the cause of the phenomenon will be discussed below.

That lead compounds should still be employed in the treatment of wine will surprise no one familiar with the tenacity with which traditions are held by successive generations of operatives in many of the chemical arts. According to Taylor\*, "litharge was formerly much used to remove the acidity of sour wine and convey a sweet taste. Acetate of lead, or some other vegetable salt of the metal, is in these cases formed, and the use of such wine may be productive of alarming symptoms. Many years since, a fatal epidemic colic prevailed in Paris owing to this cause; . . . the adulteration was discovered by Fourcroy, and was immediately suppressed."

Beckmann, in his 'History of Inventions'†, dwells at some length on the antiquity and enduring character of the practice of neutralizing the acid which spoils wine by means of litharge. According to this author, the practice was forbidden by legal enactment in France as

\* On Poisons, p. 502 of the London edition.

† Chapter on Adulteration of Wine.

early as 1696; but a hundred years later "the art of improving wine by litharge was taught in England as a method perfectly free from danger" \*.

The sulphuric acid in the sample of wine examined by me was probably added with the view of removing the dissolved lead resulting from the previous use of litharge. It is not unlikely that the addition of the free acid was preceded by that of a solution of sulphate of ammonium.

In seeking for an explanation of the fact that a certain proportion of lead may remain dissolved in wine, even in presence of free sulphuric acid, the following hypotheses suggest themselves:—

1st. It seemed not impossible, in case a mixture of weak alcohol, dilute sulphuric acid, and sulphate of lead was left to itself for a long time, that a part of the lead-salt might be changed to sulphovinate of lead and pass into solution. This idea was sufficiently improbable in view of the known facts that dilute alcohol and weak sulphuric acid are unfit for making sulphovinic acid, and that but little, if any, of the acid can be formed, even from tolerably concentrated liquids, unless the mixture of alcohol and sulphuric acid be heated artificially. The idea was nevertheless put to the test of experiment, as follows:—

100 cubic centims. of alcohol of 59 per cent., 5 cubic centims. of oil of vitriol, and a quantity of recently precipitated sulphate of lead were placed in a stoppered bottle, and the mixture was frequently shaken during an interval of three months. The clear liquid was then decanted, diluted with water, and saturated with sulphuretted hydrogen gas. Not the slightest coloration indicative of lead was produced.

100 cubic centims. of similar alcohol mixed with sulphuric acid, sulphovinic acid, and sulphate of lead, gave no reaction for lead when tested after the lapse of three months.

2nd. Though the idea seemed highly improbable, it was still possible that the sugar in the wine might in some way exert a solvent action upon sulphate of lead. It was found, however, when 100 cubic centims. of alcohol of 59 per cent., and 5 cubic centims. of oil of vitriol, together with a quantity of sugar and of precipitated sulphate of lead, were left to themselves for three months, that the clear supernatant liquid held no trace of lead in solution. For that matter, it was found that a mixture of sulphuric acid and much sugar-water was capable of precipitating all the lead even from an aqueous solution of acetate of lead. The filtrate from the sulphate of lead thus precipitated gave absolutely no indication of lead when tested with sulphuretted hydrogen, not even when a considerable quantity of the liquid was evaporated to dryness, incinerated, treated with nitric acid, and again evaporated before applying the reagent.

3rd. The most probable hypothesis of all, however, was, that a

\* William Graham's 'Art of Making Wines from Fruit, Flowers, and Herbs.' London, sixth edition.



certain proportion of lead could be held dissolved in presence of sulphuric acid, even in an alcoholic solution like wine, by the action of various soluble alkaline salts capable of decomposing and of being decomposed by sulphate of lead; for it is a well-known fact that very considerable quantities of sulphate of lead can be held dissolved in water by means of many acetates, citrates, and tartrates, and by various other salts.

To test this idea, the following set of experiments has been carried out at my suggestion by Mr. A. H. Pearson, of Haverhill, a student in the Laboratory of the Massachusetts Institute of Technology.

A considerable quantity of dilute alcohol, of the usual strength of sherry wine (18 per cent.), having been prepared, standard solutions of acetate of lead, of sulphuric acid, and of sulphate of ammonium were made by dissolving weighed quantities of these substances in portions of the 18 per cent. alcohol. Each of the solutions was made of such strength that 500 cubic centims. of the liquid contained one-tenth of an equivalent of the salt or acid, reckoned in grammes, on the hydrogen scale.

Alcoholic solutions of several salts of ammonium and of the fixed alkalies were also prepared, as will be described below.

In each experiment, equal quantities of the standard solution of sulphuric acid, or of sulphate of ammonium, and of the saline solution to be tested, were mixed in a glass flask, and the standard solution of acetate of lead was made to fall from a burette drop by drop into the mixture until a persistent precipitate of sulphate of lead was perceived. The burette was graduated so that two drops from it were equal to one-tenth of a cubic centimetre; and the flask was constantly shaken while the drops of acetate of lead were falling into it.

The results of the experiments are as follows:—

*Acetate of Ammonium* was prepared by neutralizing ordinary acetic acid with ammonia-water; and the strong aqueous solution thus obtained was mixed with alcohol. It appeared, however, that this alcoholic solution of the acetate exerted no solvent action upon sulphate of lead; for a permanent precipitate of the latter was produced in the mixture of acetate of ammonium and normal sulphuric acid by the first drop of the standard solution of acetate of lead. The same negative result was obtained in several repetitions of the experiment, even when new portions of dilute alcohol and a second set of the standard solutions were employed.

When, however, the solution of acetate of ammonium was mixed with an equal bulk (10 cubic centims.) of the standard solution of sulphate of ammonium instead of the sulphuric acid, a considerable quantity of sulphate of lead was held in solution by it. In two distinct trials, the precipitate formed by dropping acetate of lead into the mixed solution of acetate and sulphate of ammonium continued to redissolve until 3 cubic centims. of the standard solution of acetate of lead had been added to the liquor. These 3 cubic centims. of the standard solution contained 0.1137 grm. of acetate of lead, corre-

sponding to 0.0909 grm. of sulphate of lead. To hold dissolved one part of sulphate of lead in the dilute alcohol charged with sulphate of ammonium, there was consequently required 110 cubic centims. of a tolerably strong solution of acetate of ammonium.

Still another experiment with sulphuric acid was made, by mixing 10 cubic centims. of an entirely new preparation of acetate of ammonium with a similar quantity of the standard solution of acetate of lead, and dropping the standard sulphuric acid into the mixture. No persistent precipitate was produced in this case until 5 cubic centims. of the acid had been added. This quantity of the standard acid contained 0.049 grm. of sulphuric acid, corresponding to 0.1515 grm. of sulphate of lead; hence only 33 parts of the solution of acetate of ammonium were required to dissolve 1 part of sulphate of lead. It is to be observed that the insolubility of tartrate, citrate, and succinate of lead in alcohol prevents the application of this modified form of the experiment in the examples given below. With the exception of the acetates of ammonium and sodium, none of the salts experimented with can be mixed with the acetate of lead and subsequently tested with sulphuric acid or sulphate of ammonium.

*Acetate of Sodium*, whether mixed with the normal sulphuric acid, with sulphate of ammonium, or with acetate of lead, seemed to have no solvent action upon sulphate of lead.

Neither *Oxalate of Ammonium* nor normal *Oxalate of Potassium* exerted any solvent action either in presence of the sulphuric acid or the sulphate of ammonium.

*Tartrate of Ammonium*.—Normal crystallized tartrate of ammonium was dissolved in alcohol of 18 per cent., in such proportion that 500 cubic centims. of the solution contained one-tenth of an equivalent (18.4 grms.) of the salt. 25 cubic centims. of the solution was mixed with an equal volume of the normal sulphuric acid, and normal acetate of lead was added to the mixture until a permanent precipitate was produced. To effect this result, there was required of the standard solution of acetate of lead 2 cubic centims. or 0.0758 grm. of the acetate, corresponding to 0.0606 grm. of sulphate of lead. The 25 cubic centims. of the solution of tartrate of ammonium contained 0.92 grm. of the dry salt. Hence something more than 15 parts of tartrate of ammonium are required to hold 1 part of sulphate of lead dissolved in dilute alcohol containing free sulphuric acid.

In two other experiments, where the tartrate-of-ammonium solution was mixed with the sulphate of ammonium instead of with free sulphuric acid, 3 cubic centims. of the acetate-of-lead solution had to be added before a permanent precipitate could be formed.

That sulphuric acid is a more efficient precipitant of lead in presence of tartaric acid than sulphate of ammonium was shown in another way. 30 cubic centims. of the standard alcoholic acetate of lead were mixed with an equal volume of the standard solution of tartrate of ammonium. The precipitated tartrate of lead was filtered, and the filtrate mixed with a quantity of the sulphate-of-ammonium solution. No precipitate was produced, though on the subsequent

addition of sulphuretted hydrogen a slight precipitate of sulphide of lead was formed. In a similar experiment, where sulphuric acid was substituted for sulphate of ammonium, a slight precipitate was produced by the sulphuric acid, and no precipitate could be obtained afterwards with sulphuretted hydrogen.

In two other experiments, where 5 cubic centims. of the acetate-of-lead solution were mixed with 30 cubic centims. of the tartrate of ammonium, no precipitate was produced by sulphate of ammonium in the filtrate from the tartrate of lead, while sulphuric acid gave a slight precipitate as before. In this case, however, sulphuretted hydrogen gave a slight precipitate after sulphuric acid, as well as after sulphate of ammonium.

Normal *Tartrate of Potassium* mixed with the solution of sulphuric acid exerted no solvent action on sulphate of lead.

*Succinate of Ammonium*, prepared by neutralizing a solution of succinic acid with ammonia-water, exerted no solvent action when mixed with the free sulphuric acid; but when mixed with the solution of sulphate of ammonium, 6 cubic centims. of the acetate-of-lead solution were added to the liquor before a permanent precipitate fell.

Normal *Citrate of Ammonium* was prepared by neutralizing a weighed equivalent portion of crystallized citric acid with ammonia-water. 10 cubic centims. of the solution were mixed with an equal volume of the standard sulphuric acid, and the standard solution of acetate of lead was dropped into the mixture in the usual way. No permanent precipitate was formed until 16 cubic centims. of the lead solution had been added. These 16 cubic centims. contained 0.6064 grm. of acetate of lead, corresponding to 0.4848 grm. of sulphate of lead. The 10 cubic centims. of citrate-of-ammonium solution contained 0.42 grm. of crystallized citric acid. Hence 1 part of sulphate of lead was held dissolved in the mixture of alcohol and dilute sulphuric acid for every 0.8663 part of citric acid in the liquor.

On repeating the experiment, a precisely similar result was obtained: 16 cubic centims. of the standard lead solution had to be added to the mixture of alcohol and sulphuric acid before the precipitate ceased to redissolve as fast as it formed.

In two other experiments, where, instead of free sulphuric acid, 10 cubic centims. of the standard solution of sulphate of ammonium were mixed with 10 cubic centims. of the citrate-of-ammonium solution, 30 cubic centims. of the standard lead solution had to be added in each case before any permanent precipitate formed.

*Dicitrate of Ammonium* ( $C^{12}H^0(NH^1)^2O^{14}$ ) was prepared in crystals, and 22.6 grms. of the salt were dissolved in 500 cubic centims. of the 18 per cent. alcohol. 25 cubic centims. of the solution were mixed with an equal volume of the standard sulphuric acid, and the acetate-of-lead solution was dropped into the mixture in the usual way. After the addition of 8 cubic centims. of the standard acetate of lead, a permanent precipitate was produced. These 8 cubic centims. contained 0.3032 grm. of acetate of lead, corresponding to 0.2424 grm. of sulphate of lead. The 25 cubic centims. of dicitrate-

of-ammonium solution contained 1.13 grm. of the dry salt. Hence 1 part of sulphate of lead was held dissolved for every 4.6617 parts of the dicitrate.

*Tricitrate of Potassium.*—25 cubic centims. of a standard solution of ordinary crystallized citrate of potassium mixed with an equal volume of the standard sulphuric acid gave no permanent precipitate until 2 cubic centims. of the solution of acetate of lead had been added to it.

*Sugar.*—A standard solution of cane-sugar mixed with an equal volume of the sulphuric acid gave a permanent precipitate on the addition of the first drop of the acetate of lead.

These experiments show clearly that very considerable quantities of sulphate of lead can be held in solution by weak alcohol charged with various salts. It may therefore reasonably be inferred that wines sometimes retain lead in solution in consequence of this action of the acids and salts peculiar to wine upon lead compounds ignorantly employed to correct acidity.—*Communicated by the Author, from Proceedings of the American Academy of Arts and Sciences, November 11, 1868.*

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ON AURORAL APPEARANCES AND THEIR CONNEXION WITH THE  
PHENOMENA OF TERRESTRIAL MAGNETISM. BY BALFOUR  
STEWART, F.R.S., F.R.A.S.

Some years since, I ventured to suggest that auroral displays might be secondary currents due to small but rapid changes, caused by some unknown influence, in the magnetism of the earth. In developing this idea, the earth was compared to the core of a Ruhmkorff machine, and the moist upper strata of the earth, as well as the upper strata of the atmosphere, to secondary conductors, in which currents will take place whenever the magnetism of the earth changes from any cause. These views would appear to be confirmed by the very interesting records of earth-currents obtained by Mr. Airy at the Greenwich Observatory, in which it is found that during times of very great magnetic disturbance there are strong earth-currents alternating from positive to negative, the curves lying nearly equally on both sides of the zero.

A further development of this idea has lately occurred to me, in consequence of a remark of my friend Mr. Lockyer, that the zodiacal light may possibly be a terrestrial phenomenon, and may therefore be somehow connected with the phenomena of terrestrial magnetism. For not only will secondary currents be caused in a stationary conductor in presence of a magnetic core of variable power, but also in a conductor moving across the lines of force of a constant magnet. The question arises, have we on the earth such moving conductors? In answer to this, let us reflect what takes place at the equator. When once the anti-trades have reached the upper regions of the atmosphere, they will become conductors from their tenuity; and as



they pass rapidly over the lines of the earth's magnetic force we may expect them to be the vehicles of an electric current, and possibly to be lit up as attenuated gases are when they conduct electricity. May not these form the zodiacal light?

Such moving currents will, of course, react on the magnetism of the earth. We may therefore suppose that somewhat sudden and violent changes are likely to take place in the earth's magnetism at those seasons at which the earth's great wind-currents change most rapidly. May not this account for the excess of disturbances at the equinoxes?

Besides the anti-trades there are also, no doubt, convection-currents, caused by the daily progress of the sun, taking place in the upper regions of the earth's atmosphere. May not these also be the vehicle of currents as they cross the lines of the earth's force, and account, to some extent at least, for the daily variations of terrestrial magnetism? and may not this be the reason of the likeness observed by Mr. Baxendell between the curves denoting the daily progress of the wind and those denoting the variation of the declination magnet? Such currents (in as far as they are electric conductors), taking place in the upper regions of the atmosphere, would not be felt by the earth-current wires at Greenwich, and I think Mr. Airy has noticed that this is the case. But the tidal wave represents a motion of a conductor on the earth's surface, with two periods in one lunar day. This motion cannot produce a very great secondary current; but may it not be sufficient to account for the lunar-diurnal magnetic variation, which is also very small?

Such a current taking place in a conductor electrically connected with the earth's upper surface ought to be felt by the Greenwich wires; and, if I am not mistaken, Mr. Airy has detected a current of this nature.

May we not also imagine that there are two varieties of aurora—one corresponding to stationary conductors under a very rapidly changing core, and the other to rapidly moving conductors under a constant core? And might not an aurora of the latter kind indicate the approach of a change of weather?

These remarks are thrown out in order to invite comment and criticism, and they will have served their purpose if they direct attention to the part that may be played by moving conductors in the phenomena of terrestrial magnetism. It will be noticed that these remarks do not touch upon the mysterious and interesting connexion believed to exist between magnetic disturbances and the frequency of solar spots.

P.S.—Since writing the above, Sir W. Thomson has called my attention to a paper by him in the *Philosophical Magazine* for December 1851, in which it is suggested that moving conductors may play a part in the phenomena of terrestrial magnetism,—*Monthly Notices of the Royal Astronomical Society*, December 10, 1869.

THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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MARCH 1870.

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XXI. *On the Deadening of the Sounds of Solid Bodies by Internal Resistances.* By Dr. E. WARBURG\*.

IT is a well-known fact that the sound-vibrations of solids gradually become extinguished when they are not sustained by external force. The causes of this extinction are partly external—resistance of the surrounding medium, friction against the points at which the bodies are fastened, and communication of motion to them—and partly internal, depending on the nature of the solid itself. The existence of such internal causes was originally demonstrated by Wilhelm Weber in the torsion vibrations of a silk thread; for he found that they became extinguished in a short time even *in vacuo*. There can here be no appreciable communication of motion to the point of suspension, so that the forces which in this case produce the extinction must be sought in the structure of the vibrating thread.

The forces which, acting in the interior of solids, bring about the extinction of their vibrations, will be denoted in the sequel as the *internal resistance* of solids.

In reference to the internal resistance of solids, Helmholtz remarks†:—"More perfect elasticity seems peculiarly to favour the continuance of the higher tones; for more rapid vibrations are in general more quickly deadened by imperfect elasticity and friction than slower ones."

The author is not aware of any experimental proof that higher

\* Translated from the *Berliner Monatsbericht* for July 1869.

† *Lehre von den Tonempfindungen*, p. 122.

tones are more strongly deadened by internal resistance than deeper ones, nor of any investigation of the cause of this phenomenon.

The first part of this research contains experimental proofs of the phenomenon in question, the second part an investigation of the causes which produce it.

### PART I.

When a bar of any material is connected with a solid source of sound, sound-waves are transferred to it. If the duration of the tones which the source of sound emits is adequate as compared with the time within which the waves traverse the bar (which is the case in the following experiments), direct and reflected waves frequently pass through one another, and the bar assumes a condition of stationary vibrations. The forward and back waves, however, undergo a diminution of their amplitude in consequence of the deadening forces which act upon the vibrating particles; and owing to this, the intensity of the motion of sound in the bar diminishes with increasing distance from the source of sound.

Hence the author, in order to investigate the action of the deadening forces of solids upon tones of varying heights, conducted to the ear, through bars of various material, the sound of an instrument which simultaneously emitted tones of varying height; the instrument in question was a musical box. It was before all things necessary that the sound should reach the ear through the conducting-rod only. The following method of insulation was adopted with this view.

A circular cylindrical bag of thin caoutchouc 250 millims. in height, open above and filled to the top with water, was suspended in a thick cylindrical glass vessel by drawing the edge of the bag over the rim of the glass cylinder.

When the musical box, protected by a caoutchouc case and suspended by a thread, was sunk nearly to the bottom, the sound could not at all be heard by a bystanding observer; only by holding a funnel close over the edge of the water could a few of the highest notes be heard; these, however, were so feeble that the height of the note could not be clearly distinguished.

The sound of the box is powerfully imparted to the water, which results, among other things, from the circumstance that, when sunk in a glass vessel filled with water, it communicates a powerful motion to the surrounding media through the intervention of the water and the glass. The sides, too, of the bag are set in powerful vibration; for if the bag be freely suspended in the air, a considerable amount of sound is imparted to it. As in this case the free air, so also, when the caoutchouc bag is suspended in

the cylinder, the volume of air between the glass and the bag is powerfully agitated by the sides of the latter; but as this volume of air does not communicate with the external air, and as, moreover, it cannot appreciably agitate the thick glass sides, none of the motion imparted is transmitted to the surrounding air.

If while the bag is freely suspended (the musical box oscillating at some depth) the ear be moved along the sides, a great decrease of the sound is observed as the ear is moved upwards. This experiment gives the reason why so little sound reaches the air through the surface of the water. Suppose a vessel, open at the top and with absolutely rigid sides, filled with water, and that in any part whatever an impulse be given to the water (by a sounding body for instance), this impulse will be transmitted in all directions, but, owing to reflection from the rigid sides, escape principally at the free surface. But if the sides are pliable they will give way laterally, and in proportion to this pliability will impart a certain amount of motion to the air; hence, if the vessel is deep enough and the impulse be produced in a deep part, only a small amount of motion will reach the free surface of the water.

The conductors to be investigated are suitably connected at one end with the musical box suspended in water; the other end is fastened to a sounding-board, to which the ear is applied.

In this way the sound of the box was transmitted to the ear through a caoutchouc rod 460 millims. in length and 6 millims. in diameter: the deep accompaniment of the small piece of music which the box played was exclusively heard. If various parts of the rod be examined, it will be found that only near the source of sound is any thing to be perceived of the highest notes.

To investigate whether the resistance of the air has an influence on these phenomena, the musical box was suspended by a strip of caoutchouc in a vacuous vessel. Through the point of suspension and the vessel deep tones only reached the support; when the caoutchouc rod was replaced by a lead tube, high as well as deep tones were heard by an observer who applied his ear against the support.

Hence it must be concluded that the weakening of the tones by conduction through caoutchouc does not arise from the resistance of the air, but has its foundation in the nature of the caoutchouc itself.

Rods of wood, steel, glass, lead, wax, of the same dimensions as that of caoutchouc, propagated high and deep tones in sensibly equal proportion. Even in the conduction of sound through a slightly stretched copper wire 0.2 millim. in diameter and 30 metres in length, no difference could be discriminated in the propagation of high and of deep sounds. But when a lead wire  $1\frac{1}{2}$  millim. in diameter and 11 metres in length was interposed between the source of



sound and the ear, nothing more could be perceived of the higher notes, while the deep accompaniment was perfectly distinct.

A slightly stretched hemp cord 4.5 metres in length exhibited the same deportment as the caoutchouc and the longer lead conductor; when it was somewhat more tightly stretched, the higher notes were superadded to the lower ones; the caoutchouc rod, on the contrary, had to be very strongly stretched (that is, drawn out to about three times the length) for the highest tones to be propagated in it to somewhat greater distances.

Connected with these experiments is the unequal enfeeblement which notes of various height undergo on conduction through the air which is enclosed in caoutchouc tubes. Near to that end of a wooden rod connected with the musical box which projected from the water, one end of an open glass tube was approached. When the other end of the tube was introduced into the ear, the entire piece of music was heard, with the exception that, especially with short tubes, certain notes were rendered particularly prominent by resonance. When the glass tube was replaced by a caoutchouc tube, provided the length of the conduction was adequate, only the deeper notes could be perceived. For the same thickness of the sides of the caoutchouc tubes, the enfeeblement of the tones on conduction, especially of the higher ones, diminishes with a decrease in the internal diameter. To show this, it is sufficient to introduce simultaneously into the two ears two tubes of different diameters, and to bring the free ends near the source of sound. If now either one or the other tube be compressed, the difference in the strength and composition of the sound propagated by the two conductions can be judged, and it will be ascertained that the higher tones pass more readily through the narrow tube than through the wider.

This deportment shows that the enfeeblement of sound in these experiments must first of all be ascribed to imperfect reflection from the caoutchouc sides; for, *ceteris paribus*, they yield to a greater extent the greater the internal diameter of the tube. It was therefore to be expected that a considerable degree of sound was to be imparted by conduction to the external air, of which the author has convinced himself by special experiments.

We must accordingly conceive the conduction through air in caoutchouc tubes to take place by the caoutchouc tube being made to vibrate transversely by the vibrations of the air. These transversal vibrations of the solid caoutchouc are enfeebled as they advance, and, according to the experiments first described, the higher-tone vibrations much more rapidly than the deeper ones. Hence the higher tones are lost from the system more rapidly than the lower ones, and the deeper ones are maintained for a longer time. The question remains open whether also there

is a difference in the intensity of the reflection by the caoutchouc sides of sound-vibrations of different heights.

Helmholtz\* explains in a similar way the softer tone of wooden as compared with metal pipes, "inasmuch as the sides of the former do not so well resist agitation by the sound-waves, by which the higher-sound vibrations seem more easily destroyed by friction."

Hence the phenomena in the conduction of sound through the air in caoutchouc tubes are to be referred to the phenomena of the conduction of sound through solid caoutchouc; and the question now arises as to the explanation of the unequal enfeeblement of sounds of different heights when conducted through solids.

## PART II.

When vibrations are entirely and alone sustained by *elastic forces*, a diminution of the wave-length (vibrating portion), and therefore with an increase of the mean molecular displacement for the same amplitude of maximum vibration, must be inseparably connected with a diminution of the duration of vibration in one and the same body. Hence the author, in order to separate these two influences, has *combined magnetic* with elastic forces, and has had recourse to torsion vibrations sufficiently prolonged to be capable of definite measurement. If a magnet be suspended by a thread, only by changing the directive force of the magnet by another magnet suitably placed can we alter the duration of the torsion vibrations of which this system is capable. The thread, on the other hand, can be shortened, and the change in the time of oscillation of the system thus produced can be compensated by a suitable displacement of the external magnet.

This idea was executed by means of a kind of torsion-balance. To be able to alter at will the length of the threads, they were fastened at the top to a rod which could be moved vertically in the tube; at the other end was a scale-beam which had a groove to hold the magnet, and was moreover provided with a silvered vertical glass mirror. In this system, suitably protected against disturbance, torsion vibrations could be produced by means of external magnetic influences. Through a plane parallel glass inserted in the vessel of the balance, the deflections of the beam could be observed by means of a scale and telescope. Caoutchouc threads and fine silk, glass, and metal threads were investigated.

It was ascertained in this manner that within elongations of  $6^\circ$  and  $2^\circ$  from the position of equilibrium, to which limits the observations were restricted, the series of deflections may be

\* *Lehre von den Töneempfindungen*, p. 153.

very accurately represented by a geometrical series—a law of decrease which Gauss and Weber had already observed for thin metal and silk threads, and which by the author's experiments is extended to caoutchouc threads 1 sq. millim. in section. When the exponent of the series (whose logarithm is the logarithmic decrement) was suitably deduced from a number of observed deflections, and by its means another number of deflections was calculated, the differences of the observed and calculated values were throughout within the limits of possible errors of observation, which, corresponding to one division of the scale, amounted to  $\frac{1}{138}$  of the smallest and  $\frac{1}{16}$  of the greatest elongation.

Hence the motion of the system is represented by the formula

$$x = A \cdot e^{-\epsilon t} \cos nt,$$

in which

$x$  is the elongation from the position of equilibrium in angular degrees,

$A$  the elongation at the time  $t=0$ ,

$n$  the number of vibrations in the time  $2\pi$ ,

$\epsilon$  is a magnitude which is a constant for each experiment.

In this formula the magnitude  $\epsilon$  is inversely proportional to the time within which the amplitude is reduced from  $a$  to  $\frac{1}{n} a$ , and is the *measure of the deadening*. The law above-mentioned, as is known, presupposes a deadening force proportional and opposite to the velocity; *the measure of the deadening force* referred to the unit of velocity is the product  $\epsilon M$ , in which  $M$  is the moment of inertia. Now, as in these experiments the length only of the threads was changed, the balance with its belongings being always the same, the moment of inertia was sensibly constant in all the experiments. Hence the magnitude  $\epsilon$  may be considered a measure of the actual deadening, as well as a measure of the deadening force.

The relative magnitude of the deadening force (to ascertain which was the object of these experiments) might be deduced by directly observing the time within which the amplitude of  $a$  is reduced to  $\frac{1}{n} a$ . That magnitude may more certainly be deduced from the logarithmic decrement by dividing it by the duration of vibration. In this manner the author determined the magnitude  $\epsilon$ .

It was the first aim of the author to investigate the dependence of the deadening on the duration of vibration; and with this view observations were first made with the space filled with air. These experiments gave the following result:—For caoutchouc

threads the deadening increases in general considerably with increase in the duration of vibration.

Only, for the longest caoutchouc thread examined (of 328 millims.) no increase occurred for small values of the duration of vibration, but even a small decrease.

All the other threads investigated (fine silk, glass, metal threads) exhibited the same deportment, in reference to all periods of vibration examined, as did this longest caoutchouc thread for small values of the duration of vibration; for in all was there a decrease in the deadening with increase in the duration of vibration.

This complication of the phenomena might arise from the circumstance that  $\epsilon$  (the magnitude observed) is a compound one. The deadening forces to which the vibrating system is subject are partly to be sought outside it, in the resistance of the air, and partly within it, in the thread itself. The results obtained seemed to indicate that both parts of which the deadening consists varied inversely as the duration of vibration. This supposition was confirmed by experiments being made *in vacuo*. The apparatus was suitably changed for this purpose, so that the pressure of the air could be reduced to 5 millims., and during one experiment did not increase by more than 2 millims. From this modification\* it resulted that:—

1. In the case of all threads, that part of the deadening ( $\gamma$ ) arising from the internal resistance increases with the increase of the duration of vibration. There was obtained, for instance:—

*Metal.*

$\tau$	$\gamma$
8.7 . . . . .	0.000606
3.7 . . . . .	0.000498

*Caoutchouc.*

4.3 . . . . .	0.000843
3.8 . . . . .	0.000727

Hence, with the same length of thread, the slower vibrations are more powerfully deadened by the internal resistance than the quicker ones.

2. That part of the deadening ( $\alpha$ ) arising from the resistance of the air decreases with increased duration of vibration  $\tau$ . There was obtained, for instance:—

$\tau$	$\alpha$
8.7 . . . . .	0.000309
3.7 . . . . .	0.000535

\* The method used of eliminating the resistance of the air is not strictly correct according to O. E. Meyer (Pogg. Ann. vol. cxxv. p. 576 *et seq.*), but is sufficient for the present object.



Hence, by the resistance of the air, the quicker vibrations are more powerfully deadened than the slower ones.

It must be remarked in reference to (2) that, according to Stokes\*, the quicker vibrations of solids in air are more powerfully deadened than slower ones.

The first result recalls to mind the view of W. Weber† as to that cause of the decrease in the amplitude of oscillation which has its origin in the nature of the body itself. W. Weber shows that the phenomenon which he discovered, the elastic reaction, must produce a diminution in the amplitude of vibration. Now it is *à priori* probable that this reaction must manifest so much the stronger effect the slower the vibrations—which is in accordance with the author's experiments, according to which the slower torsion vibrations of a thread are more powerfully deadened by the internal resistance than more rapid ones.

We have hitherto been concerned alone with the dependence of the deadening arising from the internal resistance on the time of vibration, the length of the thread remaining constant. To ascertain the dependence of this deadening on the length of the thread, it is unnecessary to eliminate the resistance of the air; for an alteration in the deadening with a change in the length of the thread, the time of vibration remaining constant, can only be ascribed to the thread. It has now been found that the deadening increases with a decrease in the length of the thread—that is, that, for the same duration of vibration, shorter threads produce stronger deadening than longer ones.

To apply vibration-experiments to experiments on the conduction of sound, the assumption must be made that the deadening arising from the internal resistance, in the sound-vibrations of solids, must depend in the same manner on the duration of vibration and the magnitude of the vibrating divisions as has been established in the case of slow torsion vibrations.

On this assumption, the reason that the higher tones when conducted through solids are more powerfully deadened than the deeper ones cannot arise from the fact that with the higher notes the vibrations are more rapid; for, according to the vibration-experiments, it is just the slower vibrations which for the same wave-length are more rapidly deadened by the internal resistance than the quicker ones. This cause can only be that, in the higher notes, smaller vibrating divisions (wave-lengths) are formed; for, according to the vibration-experiments, a greater deadening force is developed in shorter waves (for the same duration of vibration) than in longer ones‡.

\* Transactions of the Cambridge Philosophical Society, vol. ix. part 2.

† Pogg. Ann. vol. xxxiv.

‡ To this must be added that the mass to be moved is less in the case

As in one and the same body higher tones correspond to smaller wave-lengths, so in two different substances the smaller velocity of sound corresponds for the same pitch to smaller wave-lengths. Hence, of two different bodies, for the same specific internal resistance and under circumstances in other respects similar, the deadening of tones of equal height must be greater for the body with the smaller velocity of sound.

In the vibration-experiments all the threads used exhibited qualitatively the same deportment in reference to deadening; in the experiments on the conduction of sound, a difference was so far observed in the behaviour of the various conductors used, that only in a small number (caoutchouc rod, slightly stretched hemp cord, thin lead wire) was there any difference in the deadening for the higher and lower tones. This may have arisen from the fact that the specific internal resistance in the case of the other bodies of which the conductors were formed had too small a value. Probably, however, it only arose from the fact that the velocity of propagation of the waves in the other conductors was too great. Hence, with an increase in the velocity of propagation of the waves in the experiments on the conductivity of sound, the difference in the intensity of sound at the two ends of the conductor must decrease, for two reasons:—first, according to what has been above said, because with the velocity of propagation the wave-lengths increase; and, secondly, because with an increase in the velocity of propagation, for the same length of path which conducts the sound, the time during which the deadening influences act is diminished.

## XXII. *Notes on Professor Fleeming Jenkin's Formula.*

By THOMAS T. P. BRUCE WARREN\*.

THIS formula was, I believe, first published in the 'Cantor Lectures' in 1865, but, beyond insertion, has received no notice in works on telegraphy; even by electricians, generally, it appears to have been treated as of little or no importance.

The object of this paper is to point out some of the results arising from its application to cable-testing.

In the formula

$$R = \left( \frac{t}{K \times \log_e \frac{C}{c}} \right) 10^6,$$

of a smaller vibrating division than in that of a larger one; so that for a double reason the ratio of the *deadening force to the mass moved*, on which the deadening depends, has a greater value for the shorter waves than for the longer ones.

\* Communicated by the Author.

$R$  = resistance in B.A. units,

$t$  = time in seconds during which  $C$  falls to  $c$ ,

$K$  = capacity in farads,

$C$  = charge (full tension),

$c$  = charge remaining at end of time  $t$ .

I showed in a former communication that  $R$  will not be the exact resistance of a core or cable at the end of time  $t$ , in consequence of electrification on charging, and that this electrification will be proportional to the loss of charge during the first unit of time.

The first proposition is to show that the rates of loss for static charge, and also the resistances, are as the Napierian logarithms of  $\frac{C}{c}$ , and not as the simple multiples of  $\frac{C}{c}$ .

First ( $t$  and  $K$  being the same in both cases), let  $\frac{C}{c}$  correspond to a loss of 5 per cent.

Let  $t = 60$  seconds,  $K = \cdot 5$  farad,

$$R = \frac{60}{\cdot 5 \times \cdot 04980} = 2449 \text{ millions.}$$

Secondly, let  $\frac{C}{c}$  correspond to a loss of 20 per cent., or four times as great in the same time  $t$ ,

$$R = \frac{60}{\cdot 5 \times \cdot 22314} = 538 \text{ millions.}$$

Thus, although the loss is four times as much in the one as in the other, the resistance is 4.55 times less; that is, the resistances are not proportional to the simple multiples of loss in the same time  $t$ .

This may appear at first sight anomalous, but can be shown to be correct as follows; and from a general consideration of the formula for loss of static charge,

$$\frac{\log \frac{C}{c}}{\log \frac{C}{c'}} n = nt,$$

where  $C$  is the full tension,  $c$  the charge remaining at end of time  $t$ , and  $c'$  the required charge at end of  $n$  times  $t$ . If, then, a core lose 5 per cent. of its charge in one minute, we shall find that it will require (4.55 times  $t$ ) 4.55 minutes to lose 20 per cent; or, conversely, a core losing 20 per cent. of its charge in

one minute, will require 13·2 seconds to lose 5 per cent.; and these values of  $t$  will be proportional to the Napierian logarithms of  $\frac{C}{c}$ , and therefore to their resistances; for

$$\frac{60}{\cdot 5 \times \cdot 04908} = \frac{273}{\cdot 5 \times \cdot 22314} = 2449 \text{ millions,}$$

and

$$\frac{13\cdot 2}{\cdot 5 \times \cdot 04908} = \frac{60}{\cdot 5 \times \cdot 22314} = 538 \text{ millions.}$$

From this it is evident that, for reliable comparisons between cores or cables which have different rates of loss, the times should be noted when they each lose the same proportion of charge. It is incorrect to state the comparison by percentages of loss for the same time.

In the formula  $K$  is a function of length and thickness of dielectric  $\left(\log \frac{D}{d}\right)$ , so that,  $\log \frac{D}{d}$  being constant,  $R$  will vary as the length of the cable or core; consequently if  $K$  represent the total capacity of a long length of cable, or that of a single mile of the same cable,  $R$  will be the resistances of these respective lengths.

Suppose a loss of 5 per cent. in these cases; and first, the length being one mile,  $K$  being the same for each unit of length and equal to  $\cdot 5$  farad,  $t$  being one minute,

$$R = \frac{60}{\cdot 5 \times \cdot 04908} = 2449 \text{ millions.}$$

Next, let the length be 1000 miles;

$$R = \frac{60}{\cdot 5 \times 1000 \times \cdot 04908} = 2\cdot 449 \text{ millions,}$$

and

$$2\cdot 449 \times 1000 = 2449 \text{ millions per mile.}$$

Hence in the formula  $K$  enters as the function of length in terms of the capacity per unit of length.

The next proposition is to show that whatever interval  $t$  be taken, if the correct loss corresponding to that time be known, the value for  $R$  is constant.

The 1866 Atlantic Telegraph cable fell to half tension in 66 minutes 40 seconds.  $K = \cdot 3535$  farad per nautical mile. Length = 1896 nautical miles.

$$R = \frac{4000}{\cdot 3535 \times 1896 \times \cdot 69314} = 9 \text{ millions (nearly),}$$

$$9 \times 1896 = 17,000 \text{ millions per nautical mile.}$$



The calculated loss corresponding to one minute is rather more than 1 per cent. (1·0096) ;

$$R = \frac{60}{\cdot 3535 \times 1896 \times \cdot 00995} = 9 \text{ millions (nearly),}$$

$$9 \times 1896 = 17,000 \text{ millions per nautical mile.}$$

Mr. Latimer Clark says (Culley's 'Handbook,' p. 231) that the Atlantic cable when laid had a resistance equal to 2437 millions B.A. units per nautical mile, which after 30 minutes' electrification rose to 7000 millions. We must therefore consider the cable to have been highly electrified when Mr. Clark's electrometer tests were taken. The calculated fall in these tests does not agree so well with the observed fall as may be wished ; for whatever be the interval for  $t$ ,  $R$  will be constant if the correct value of  $\frac{C}{c}$  corresponding to the time  $t$  be taken.

Thus, the fall to half tension being 4000 seconds, we find by

formula  $\frac{\log \frac{C}{c}}{\log \frac{C}{c}} n = nt$ , that 1640 seconds are required to fall to three-fourths (Mr. Clark's tests give 1363 seconds) ;

$$R = \frac{1640}{\cdot 3535 \times 1896 \times \cdot 28540} = 9 \text{ millions (nearly),}$$

$$9 \times 1896 = 17,000 \text{ millions per nautical mile.}$$

And in general, whatever may be the amount of electrification given to a core,  $R$  will be constant for all values of  $\frac{t}{\log_e \frac{C}{c}}$ , where

$t$  is the time or intervals in which  $C$  falls to  $c$  ; for the electrification given on charging reduces the rate of loss exactly in the same ratio as it increases the resistance.

The value for  $\rho$ , as written in my modification of this formula, must in the first instance be obtained by experiment ; and it must be understood that  $t$  is the time for which a core is held free, and that no provision for the time occupied in charging is made without introducing the value corresponding to it for the period of contact.

When several coils having different rates of loss are joined up, the rate of loss for each in the same unit of time being known, the

rate of loss on the whole for the same unit of time may be ascertained by adding together the Napierian logarithms of the ratios  $\frac{C}{c}$  of the several coils and dividing by the number of coils, or  $\log_e \frac{C}{c}$  of the whole will be the mean of the Napierian logarithms for  $\frac{C}{c}$  of each coil, from which the rate of loss may be easily found.

In ascertaining R by formula  $R = \frac{t}{K \times \log_e \frac{C}{c}}$ , this new value for  $\frac{C}{c}$  is taken, and K is made equal to the sum of the capacities of the several coils. We may arrive at the same result by using the formula for the derived circuit,

$$R = \frac{\frac{t}{K \times \log_e \frac{C}{c}} \times \frac{t}{K' \times \log_e \frac{C}{c}} \times \frac{t}{K'' \times \log_e \frac{C}{c}} \times \&c.}{\frac{t}{K \times \log_e \frac{C}{c}} + \frac{t}{K' \times \log_e \frac{C}{c}} + \frac{t}{K'' \times \log_e \frac{C}{c}} + \&c.}$$

K, K', K'' are the capacities of the several coils. In this case it is not necessary that  $t$  should be the same unit of time. The periods of charging must in all cases be of the same duration.

If at any temperature the rate of loss in time  $t$  be known, and we require the rate of loss for the same core or cable at any other temperature and for the same unit of time, we have to raise the temperature-coefficient to the power expressed by the difference between the temperatures (as in correcting dielectric resistances), which, multiplied into the Napierian logarithm of the known rate of loss  $\left(\frac{C}{c}\right)$ , will give the Napierian logarithm of  $\frac{C}{c}$  corresponding to the required loss.

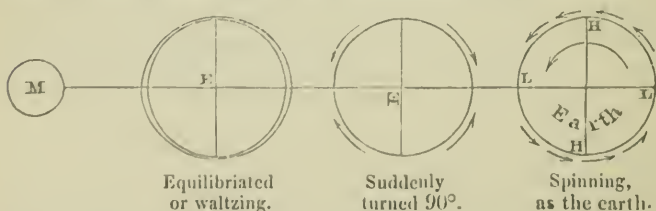
As the resistances are proportional to the times occupied in losing equal percentages of charge, we may also obtain the loss at any required temperature by knowing only the resistances and the loss in one case only.

XXIII. *Popular Difficulties in Tide Theory.*

By E. LACY GARBETT\*.

THE paradoxical fact that, on a rapidly rotating earth, the places of high and low water (friction apart) are contrary to those that would exist on a non-rotating earth, seems brought by Professor Abbott for the first time within easy popular apprehension<sup>1</sup>—which was necessary to remove a certain stigma of unreality from the current elementary teaching about tides. This lucid explanation seems to be such as may enable a child to see that if the waters have their diurnal rotary velocity increased and diminished in the four quadrants, according to the arrows of his diagram, the result must be “low water under the moon,” or high water at moonrise and moonset, and earlier the more friction. But the popular difficulty is now the antecedent one, to make clear (what is, of course, familiar to all who have considered perturbations) that those arrows do express the directions of tangential force, not only in the hemisphere next the moon, but in the other also.

The only simple way to do this, appears to be by recurring to the old diagram of an *equilibrated* earth, with its two high waters, one towards or *under* the moon, and one away from or *over* the moon; where the consideration of the decrease of lunar attraction from the nearer surface to the centre, and thence to the further surface, makes clear that on a globe behaving to the moon as the moon does to us (which Mr. Denison well calls “waltzing”), this is the only figure the waters could maintain; the summits being where, on one side, they are literally most *pulled from the earth*, and on the other, *the earth pulled from them*.



Let such a globe with its waters be supposed instantaneously turned, in either direction, a quarter round. The waters, to resume their figure relatively to the moon, must make horizontal movements in the directions of the four arrows, and with velocities whose maximum, at the middle of each quadrant, is calculable, and may be shown, in the most elementary way, to

\* Communicated by C. Tomlinson, F.R.S.

<sup>1</sup> Phil. Mag. Jan. 1870, p. 49.

be trifling compared to the earth's rotation. Now let the earth and waters rotate together eastward, as they do, with a speed immensely exceeding these waterflows, and it is seen they must act as accelerations in the quadrants coming to the moon's meridian, and retardations in those leaving it. The separation and crowding of the arrows in the third earth-figure may, therefore, express the water's rotary speed, exceeding the earth's in the nearest and furthest quadrants, and falling short of it in those of moonrise and moonset; whence the new places of high and low water, just contrary to those in the equilibrated earth, become plain. For a child must see that, as in pouring out treacle the downward acceleration makes the stream grow thinner, and in an upward jet retardation makes it thicker, so here the sea, wherever its rotation is being accelerated, must grow shallower (that is, the tide be falling), and the contrary where its rotation is being retarded.

But if, fortified by the names of Laplace, Airy, and Abbott, we grant this result to be correct, will it not follow that, seeing an equilibrated earth would have (though no tides, and indeed no moonrise or moonset) yet a permanent *high* water under the moon, while the earth with her actual rotation tends (friction apart) to have *low* water permanently under the moon, there must be an intermediate state, a rate of rotation quicker than monthly but slower than daily, that would cause no lunar high or low water at all, and consequently no tidal currents nor friction? And if so, of course there is also, substituting "sun" for "moon," a rate of rotation (it may be greater or equal or less than the above) which, though producing regular sunrise and sunset, would have no solar tides. And is not the former rate, or a mean between these two (which cannot very widely differ), the final and permanent rate of rotation to which tidal friction is tending to reduce, and must ultimately reduce, our globe? If so, the length of day and night, which has passed its minimum (as Mayer said) and is now on the increase<sup>2</sup>, must lengthen, at a rate increasing to a maximum and then diminishing, till it permanently settles to a duration calculably (but not greatly) shorter than a month. And though this will give no tides, I conclude that a slower rotation would produce tides, and of such a kind that their friction would accelerate instead of retarding the earth, and give her that same final speed.

It appears that, without supposing the remark to be in anywise new, I happened in 1853 to make the first English mention that tidal friction must increase the length of the day (in the article "Steam-engine" of 'Tomlinson's Cyclopædia of Useful Arts and Manufactures'), and to suggest (what Delaunay is now consi-

<sup>2</sup> Phil. Mag. June 1863, p. 427.



dered to have verified) that this cause might have counteracted and masked the shortening due to thermal contraction, so as to account for the non-diminution (or, as now admitted, lengthening) of the day since Hipparchus's time. I did not say, as Mayer is represented to have done, that tides would "*stop* the earth's rotation"<sup>3</sup>, but only *retard* it; and of what either he or his predecessor Kant really said on the matter I was and remain quite ignorant<sup>4</sup>. As an obvious consequence of the established view of tide-causation, I supposed it common property,—the loss of *vis viva* seeming to be a plain truism, from the mere fact that corn has been ground by tide-mills, the power spent having no possible source but the earth's original rotary spin.

The chief difficulty, however, about tide theory has always struck me as lying in its involving tides obviously the greatest at the equator, where in fact they are smallest on the whole. The exceptionally high tides of the British seas, which seem equalled only in a few areas yet smaller, lead us to form far too high an estimate of the phenomenon at large, and to doubt whether Newton's deductions as to the average lunar and solar effects (say 7 and 2 feet) suffice for the known phenomena; when in fact they far more than suffice. Remembering these school common-places, and going from our high-tided shores to the Caribbean Sea, no part of which has tides exceeding, or barely exceeding, a foot, and further, learning from navigators that they regarded such barely sensible tides as general to nearly all the tropical zone, especially its Pacific half, I was puzzled to know *what becomes* of the theoretic neap waves of 5 or 6 feet, and springs of 9 or 10. In their proper nursing-ground, between the tropics, or at least the *lunar* tropics (which enclose, let us remember, as nearly as we can know, just *half the unfrozen earth*<sup>5</sup>, and probably more than half its watery surface), any approach even to this theoretic average seems, by Romme's Tables, to be known only in a few local river bores; while all the cases decidedly exceeding it, fluvial or marine, are in *high latitudes*—our whole British plateau being such an exception, perhaps the most extended of any, but as trifling on the earth's watery surface as the two extreme areas of 40 feet tide, in the Severn and near St. Malo, are upon itself. On the other hand, vast extents of the tropical Pacific must, as appears from the case of islands like the Tahiti group (said to have "always high water at noon, and low at midnight"), be sensibly *tideless*—all lunar effect being masked by the mere heaping-up and withdrawal of coast-water by day

<sup>3</sup> Phil. Mag. May 1863, p. 377.

<sup>4</sup> Phil. Mag. April 1866, p. 322, and June, p. 533.

<sup>5</sup>  $2 \sin 28^{\circ} 44'$  (being moon's greatest declination) =  $\sin 74^{\circ}$  (the limiting latitudes of unfrozen sea).

and night breezes, in the very region where theory plainly gives the attractions freest play! Now, on reviewing the popular expositions, I presently saw a flaw whose correction would just tend, and is the only thing I can conceive tending, to explain this general fact—the confinement of all extra large, and even of the majority of decidedly sensible tides, to high, or at least extra-tropical latitudes.

In all theories the “solid” mass of the earth is regarded as *absolutely* unyielding to the forces that deform its waters. This involves attributing to it not merely solidity, but a wholly *imaginary* strength or degree thereof, one of whose existence matter nowhere gives us any trace of evidence. A glance at experiments on the strongest materials, made for engineering data, shows how small, compared with geographic distances, is the highest precipice, for instance, that any known solid could form without crushing its own base:—for ice, it is said, but 710 feet<sup>6</sup>; chalk and freestone little more than the highest of our cliffs of them; and no stone or metal probably over one mile. In the experiments of M. Tresca, cold metals *flow* perfectly under pressures less than exist at the first league or two underground<sup>7</sup>; so that a few miles below us an unfilled cavity could no more sustain itself in *cold* iron or *cold* granite than in water. Now, is a body transmitting pressure in all directions, or wherein no cavity can exist, to be called a solid or fluid? In short, what definition of a “solid” can we give apart from scale?<sup>8</sup> Is not cold butter or cold asphalt a solid on the scale of inch cubes, and a liquid on the scale of the “pitch lake” in Trinidad? Viewed on a terrestrial scale, what is the practical difference between the “solidity” of calvesfoot jelly, whereof a precipice 3 feet high might stand, and that of granite, wherein one of three miles might perhaps be possible? It is a question, you see, of the depth of “*superficial viscosity*,” as Van de Mensbrugghe and Mr. Tomlinson would say. The “solid” globe that all tide theories assume, then, is seen to require some form of matter bearing to quartz or diamond the relation that these bear to water; in short, we are assuming what inductive reasoning has no right to, something *supernatural*, *i. e.* a quality exalted to a degree not approached in *known* nature. With *her* solids, or the utmost solidity *known of*, we can get no globe whose behaviour to the tidal forces will differ otherwise from that of the sea than in

<sup>6</sup> Phil. Mag. January 1870, p. 6.

<sup>7</sup> Phil. Mag. September 1865, p. 240.

<sup>8</sup> Phil. Mag. February 1860, p. 96. Prof. Challis says, “The mass of the earth, taken as a whole, must be regarded as a fluid. . . . The effect of internal pressure would cause the distinction between solidity and fluidity to cease, probably at no great depth.”

being more viscous, having a stiffer and deeper "surface viscosity," this being a surface property in all bodies, whether it have a degree that leads us to call them solid or only fluid<sup>9</sup>.

All this is apart from plutonic heat, and would apply if we knew the temperature to be icy all through. Of course there are two quite general facts (earthquakes and strata-dislocations) that disprove the existence of any "*crust*," a term implying something capable, in parts at least, of *self-support* as a vault or shell. The "solidity" really belongs to what can but be compared to the silk of a balloon, or the finest grapeskin; and talking, in the face of geological facts, of mathematically deducing from precession or nutation, an adamantine "*crust*," some hundreds of miles thick, is equivalent to producing mathematical proofs, when a frozen pond is momentarily being roughened with dislocatory cracks (and these only a foot long), that the ice is so many fathoms thick! The three newest dislocations of which we have accounts, those in Cutch<sup>10</sup> and that in north New Zealand<sup>11</sup>, barely extend a hundred miles each, and did not sensibly shake places within a very few degrees of them. What would be the least length of crack and area of shock in the "*crust*" supposed to be deducible from precession? To illustrate the possibility of such deductions, Mr. Denison, in his '*Astronomy without Mathematics*,' observes that a pendulum formed of a hollow globe filled with quicksilver would have a quicker rate than if solid, and by the swinging the thickness of shell might be found. I cannot see that there is any analogy; for the difference of rate would depend on the fluid slipping and escaping the *interrupted* rotary movements of the shell. But in the *continuous* rotations of the earth (partaken also by her external fluids) there is no ground for assuming a slip of any part; and if none is supposed even as to the rapid diurnal rotation, much less may it be in the slow nutation and precession movements.

In any case, however, seeing the earth must (unless we make preternatural assumptions) undergo deformation by the same forces as the sea, only as much less as a thin "surface viscosity" might oblige, I perceived that the earth-tides must be as simple as those of a hypothetic uniform sea. Their summits cannot travel out of the tropical zone, nor have we reason to assume any such anomalies as the land-barriers and varying depths cause in the water-tides. Hence, though these latter present, at different places, every variety of "establishment" from

<sup>9</sup> Phil. Mag. June 1866, p. 473. "There is therefore no evidence that in the interior of large masses like the earth . . . there can be any atomic arrangements or conditions distinguishing the solid from the fluid state."

<sup>10</sup> Lyell, '*Principles*,' tenth edit. vol. ii. p. 100.

<sup>11</sup> Lyell, *ibid.* p. 86.

0 to 12 hours, and every "age of the tide," from 1 or 2 hours about Juan Fernandez to some 60 at Tide-end-town (Teddington), the earth-tides must everywhere have sensibly the same establishment; and this between 0 and 6 hours, or almost certainly between 2 and 4. Between these limits also fall doubtless the establishments of the great majority of oceanic surface, at least within the tropics<sup>12</sup>. The main body, then, of the tides of the globe's warmer half, I regard as the mere difference between a sea and an earth-tide, and hence their smallness. There seem even large districts, quite pelagic, where the minimum of skin or "surface viscosity" allows the earth-tide so nearly to equal the marine as to leave the latter insensible, as at Tahiti, and also the Virgin Isles, where I found, at St. Thomas in 1845, not a range of four inches during the day after a full moon. And these are, as we might expect, the regions giving otherwise most evidence of instability—in the latter case the evidence from frequency of earthquakes being less striking than the numerous records of shocks, in most of the Caribbean islands, immediately following or accompanying their chief hurricanes; that is, as I suppose, the mere removal, from a wide surface, of about a fifteenth of the accustomed pressure, or two barometric inches (where at no other time is the range one-twentieth of an inch) suffices to derange the equilibrium; and in the case of the St. Thomas's hurricane of two years ago, it began a series of shocks lasting months, after still longer quiescence.

Now, on looking through Romme's Tables, we find all the intertropical cases of tides approaching an average magnitude have establishments of under 2 or over 4 hours; all that can be called large have them between 6 and 12; and all the largest between 8 and 10, in other words, the water-tide so modified from a primitive one as to make high water arrive with low earth (if I am right), and low water with high earth. The greatest lengths of coast having either such establishments, or tides approaching ours, are confined to three bays (of Bengal, Guinea, and Panama), and the correspondence of the two elements is most striking. The nearer the establishment to 9 hours, as about Panama,

<sup>12</sup> Romme, *Tableau des Vents, des Marées, &c.* i. p. 388. According to Airy, 'Tides and Waves,' art. 281, 282, "If the period of the forced tide-wave be less than that of a free wave . . . (*i. e.* if the wave be urged along more rapidly than it would go alone) . . . it is low water under the moon;" and in the contrary case, "(*i. e.* if the water were so immensely deep that the wave would travel alone more rapidly than the disturbing forces urge it along) . . . there would be high water under the moon." Now as the former is the case of our deepest oceans, but the latter that of the internal fluid abyss, or earth itself, it would follow that, *were there no friction*, the general water establishment would be 6 hours, and the *ground's* establishment 0 or 12. The general effect of friction is to make the hour of high water earlier, but of high earth later.



the greater the tides; and a few degrees north or south, where it varies to near its average of 3 hours, they are recorded as "irregular" or "barely sensible." A similar contrast occurs between the two sides of Madagascar. On its east coast, where, from a wide ocean, the sea-wave and earth-wave arrive together, the tides never anywhere equal 3 feet (or a third of Newton's and Airy's calculated average for spring tides). But on the landward side they are "considerable," and larger the nearer  $17^{\circ}$  (that is, the later their establishment, till at the above latitude it is retarded about 6 hours).

In short, I conclude the tropical tides are only differential, and become large only where the hour of high water is so retarded by local circumstances as to correspond nearly to low earth. But to high latitudes the earth-tides of course do not sensibly reach, thus leaving the derived sea-tide to acquire the magnitudes familiar to us. A good test of this view might be found in equatorial estuaries long enough to have several hours' difference of establishment—especially the Amazon, the only river I find alleged to have several tides within it at once. These ought, instead of the character of progressive waves, to show rather that of stationary or see-saw ones, their summits rising at fixed places, and having between every two a place of minimum tide, namely, where the water establishment is the same that I suppose the ground to have everywhere.

London, January 8, 1870.

XXIV. *On Ocean-currents.* By JAMES CROLL, of the *Geological Survey of Scotland*\*.

PART II.

*Ocean-currents in relation to the Physical Theory of Secular Changes of Climate.*

**D**EFLATION of *Ocean-currents the chief cause of Secular Changes of Climate.*—The enormous extent to which the thermal condition of the globe is affected by ocean-currents seems to cast new light on the mystery of geological climate. What, for example, would be the condition of Europe were the Gulf-stream stopped, and the Atlantic thus deprived of one-fifth of the absolute amount of heat which it is now receiving above what it has in virtue of the temperature of space? If the results just arrived at be anything like correct, the stoppage of the stream would lower the temperature of Northern Europe to an extent that would produce on it a condition of climate as severe as that

\* Communicated by the Author.

of North Greenland at the present day; and were the warm currents of the North Pacific also at the same time stopped, the northern hemisphere would assuredly be under a state of general glaciation.

Suppose also that the warm currents, having been withdrawn from the northern hemisphere, should flow into the Southern Ocean. What then would be the condition of the southern hemisphere? This transference of heat from the northern hemisphere to the southern would raise the temperature of the latter hemisphere about as much as it would lower the temperature of the former. It would consequently raise the mean temperature of the antarctic regions much above the freezing-point, and the ice under which those regions are at present buried would, at least to a great extent, disappear. The northern hemisphere, thus deprived of the heat from the equator, would be under a condition of things similar to that which prevailed during the glacial epoch, while the other hemisphere, receiving the heat from the equator, would be under a condition of climate similar to what we know prevailed in the northern hemisphere during a part of the Upper Miocene period, when North Greenland enjoyed a climate as mild as that of England at the present day.

This is no mere picture of the imagination, no mere hypothesis devised to meet a difficult case; for if what has already been stated be not completely erroneous, all this follows as a necessary consequence from physical principles. If the warm currents of the equatorial regions be all deflected into one hemisphere, such must be the condition of things. But how can such a deflection as this take place? Upon what principles could the warm water of the equatorial region ever have been deflected all, or even nearly all, upon one hemisphere? What are the physical agencies or conditions that could possibly affect the great currents of the globe to such an extent?

*The Physical causes which deflect Ocean-currents.*—Some of these have been discussed at considerable length on former occasions; but I shall here endeavour to state them in a more brief and concise form. A clear apprehension of them is necessary for the proper understanding of what follows in reference to the influence of ocean-currents on the distribution of plant and animal life on the surface of the globe during geological epochs.

Secular displacements from the one hemisphere to the other of the great system of ocean-currents take place indirectly from an astronomical cause. The cause to which I refer is the change in the eccentricity of the earth's orbit. Directly, the eccentricity of the orbit can have no possible influence on ocean-currents; but indirectly it has, as we shall presently see. Change of eccentricity, although it cannot directly affect the currents of the

ocean, yet brings into operation physical agencies which can and do affect such currents to an enormous extent.

With the eccentricity at its superior limit and the winter occurring in the aphelion, the earth would be 8,641,876 miles further from the sun during that season than at present. The reduction in the amount of heat received from the sun owing to his increased distance would, upon the principle we have already stated, lower the midwinter temperature, other things being equal, about  $46^{\circ}$ . In temperate regions the greater portion of the moisture of the air is at present precipitated in the form of rain, and the very small portion which falls as snow disappears in the course of a few weeks at most. But in the circumstances under consideration, the mean winter temperature would be lowered so much below the freezing-point that what now falls as rain during that season would then fall as snow. But this is not all; the winters would then not only be colder than now, but they would also be much longer. At present the winters are nearly eight days shorter than the summers; but with the eccentricity at its superior limit and the winter solstice in aphelion, the length of the winters would exceed that of the summers by no fewer than thirty-six days. The lowering of the temperature and the lengthening of the winter would both tend to the same effect, viz. to increase the amount of snow accumulated during the winter; for, other things being equal, the larger the snow-accumulating period the greater the accumulation. I may remark, however, that the absolute quantity of heat received during winter is not affected by the decrease in the sun's heat\*; for the additional length of the season compensates for the decrease in the sun's heat. As regards the absolute amount of heat received, increase of the sun's distance and lengthening of the winter are compensatory, but not so in regard to the amount of snow accumulated.

The consequence of this state of things would be that, at the commencement of the short summer, the ground would be covered with the winter's accumulation of snow.

Again, the presence of so much snow would lower the summer temperature and prevent to a great extent the melting of the snow.

There are three separate ways whereby accumulated masses of snow and ice tend to lower the summer temperature, viz. :—

*First.* The snow and ice lower the temperature by means of direct radiation. No matter what the intensity of the sun's rays may be, the temperature of the snow and ice can never rise above  $32^{\circ}$ . Hence the presence of the snow and ice tends by

\* When the eccentricity is at its superior limit, the absolute quantity of heat received by the earth during the year is about one three-hundredth part greater than at present. But this does not affect the question at issue.

direct radiation to lower the temperature of all surrounding bodies to  $32^{\circ}$ .

In Greenland, a country covered with snow and ice, the pitch has been seen to melt on the side of a ship exposed to the direct rays of the sun, while at the same time the surrounding air was far below the freezing-point; a thermometer exposed to the direct radiation of the sun has been observed to stand above  $100^{\circ}$ , while the air surrounding the instrument was actually  $12^{\circ}$  below the freezing-point\*. A similar experience has been recorded by travellers on the snow-fields of the Alps†.

These results, surprising as they no doubt appear, are what we ought to expect under the circumstances. The diathermancy of air has been well established by the researches of Professor Tyndall on radiant heat. Perfectly dry air seems to be nearly incapable of absorbing radiant heat. The entire radiation passes through it almost without any sensible absorption. Consequently the pitch on the side of the ship may be melted, or the bulb of the thermometer raised to a high temperature by the direct rays of the sun, while the surrounding air remains intensely cold. "A joint of meat," says Professor Tyndall, "might be roasted before a fire, the air around the joint being cold as ice"‡. The air is cooled by *contact* with the snow-covered ground, but is not heated by the radiation from the sun.

When the air is humid and charged with aqueous vapour, a similar cooling effect also takes place, but in a slightly different way. Air charged with aqueous vapour is a good absorber of radiant heat, but it can only absorb those rays which agree with it in *period*. It so happens that rays from snow and ice are, of all others, those which it absorbs best. The humid air will absorb the total radiation from the snow and ice, but it will allow the greater part of, if not nearly all, the sun's rays to pass unabsorbed. But during the day, when the sun is shining, the radiation from the snow and ice to the air is negative; that is, the snow and ice cool the air by radiation. The result is, the air is cooled by radiation from the snow and ice (or rather, we should say, *to* the snow and ice) more rapidly than it is heated by the sun; and, as a consequence, in a country like Greenland, covered with an icy mantle, the temperature of the air, even during summer, seldom rises above the freezing-point.

Were it not for the ice, the summers of North Greenland, owing to the continuance of the sun above the horizon, would be as warm as those of England; but, instead of this, the Greenland

\* Scoresby's 'Arctic Regions,' vol. ii. p. 379. Daniell's 'Meteorology,' vol. ii. p. 123.

† Tyndall, 'On Heat,' article 364.

‡ Ibid.



summers are colder than our winters. Cover India with an ice-sheet, and its summers would be colder than those of England.

*Second.* Another cause of the cooling effect is that the rays which fall on snow and ice are to a considerable extent reflected back into space. But those that are not reflected, but absorbed, do not raise the temperature, for they disappear in the mechanical work of melting the ice. The latent heat of ice is about  $142^{\circ}$  F.; consequently in the melting of every pound of ice a quantity of heat sufficient to raise one pound of water  $142^{\circ}$  disappears, and is completely lost, so far as temperature is concerned. This quantity of heat is consumed, not in raising the temperature of the ice, but in the mechanical work of tearing the molecules separate against the forces of cohesion binding them together into the solid form. No matter what the intensity of the sun's heat may be, the surface of the ground will remain permanently at  $32^{\circ}$  so long as the snow and ice continue unmelted.

*Third.* Snow and ice lower the temperature by chilling the air and condensing the vapour into thick fogs. The great strength of the sun's rays during summer, due to his nearness at that season, would, in the first place, tend to produce an increased amount of evaporation. But the presence of snow-clad mountains and an icy sea would chill the atmosphere and condense the vapour into thick fogs. The thick fogs and cloudy sky would effectually prevent the sun's rays from reaching the earth, and the consequence would be that the snow would remain unmelted during the entire summer. In fact we have this very condition of things exemplified in some of the islands of the Southern Ocean at the present day. Sandwich Land, which is in the same parallel of latitude as the north of Scotland, is covered with ice and snow the entire summer; and in the island of South Georgia, which is in the same parallel as the centre of England, the perpetual snow descends to the very sea-beach. The following is Captain Cook's description of this dismal place:—"We thought it very extraordinary," he says, "that an island between the latitude of  $54^{\circ}$  and  $55^{\circ}$  should, in the very height of summer, be almost wholly covered with frozen snow, in some places many fathoms deep. . . . The head of the bay was terminated by ice-cliffs of considerable height; pieces of which were continually breaking off, which made a noise like a cannon. Nor were the interior parts of the country less horrible. The savage rocks raised their lofty summits till lost in the clouds, and valleys were covered with seemingly perpetual snow. Not a tree nor a shrub of any size were to be seen. The only signs of vegetation were a strong-bladed grass growing in tufts, wild burnet, and a plant-like moss seen on the rocks. . . . We are inclined to think that the interior parts, on account of their elevation, never enjoy

heat enough to melt the snow in such quantities as to produce a river, nor did we find even a stream of fresh water on the whole coast”\*.

Captain Sir James Ross found the perpetual snow at the sea-level at Admiralty Inlet, South Shetland, in lat.  $64^{\circ}$ ; and while near this place the thermometer in the very middle of summer fell at night to  $23^{\circ}$  F.; and so rapidly was the young ice forming around the ship, that “I began,” he says, “to have serious apprehensions of the ships being frozen in”†. At the comparatively low latitude of  $59^{\circ}$  S., in long.  $171^{\circ}$  E. (the corresponding latitude of our Orkney Islands), snow was falling on the longest day, and the surface of the sea at  $32^{\circ}$ ‡. And during the month of February (the month corresponding to August in our hemisphere) there were only three days in which they were not assailed by snow-showers§.

In the Straits of Magellan, in  $53^{\circ}$  S. lat., where the direct heat of the sun ought to be as great as in the centre of England, MM. Churrea and Galcano have seen snow fall in the middle of summer; and though the day was eighteen hours long, the thermometer seldom rose above  $42^{\circ}$  or  $44^{\circ}$ , and never above  $51^{\circ}$ ||.

This rigorous condition of climate chiefly results from the rays of the sun being intercepted by the dense fogs which envelope those regions during the entire summer; and the fogs again are due to the air being chilled by the presence of the snow-clad mountains and the immense masses of floating ice which come from the antarctic seas. The reduction of the sun’s heat and lengthening of the winter, which would take place when the eccentricity is near to its superior limit and the winter in aphe-  
lion, would in this country produce a state of things perhaps as bad as, if not worse than, that which at present exists in South Georgia and South Shetland.

If we turn our attention to the polar regions, we shall find that the cooling effects of snow and ice are even still more marked. The coldness of the summers in polar regions is owing almost solely to this cause. Captain Scoresby states that, in regard to the arctic regions, the general obscurity of the atmosphere arising from fogs or clouds is such that the sun is frequently invisible during several successive days. At such times, when the sun is near the northern tropic, there is scarcely any sensible quantity of light from noon till midnight¶. “And snow,” he says, “is so common in the arctic regions, that it may be boldly stated

\* Captain Cook’s ‘Second Voyage,’ vol. ii. pp. 232, 235.

† Antarctic Regions, vol. ii. pp. 345–349.

‡ Ibid. vol. i. p. 167.

§ Ibid. vol. ii. p. 362.

|| Edinburgh Philosophical Journal, vol. iv. p. 266.

¶ Scoresby’s ‘Arctic Regions,’ vol. i. p. 378.

that in nine days out of ten during the months of April, May, and June more or less falls ”\*.

On the north side of Hudson’s Bay, for example, where the quantity of floating ice during summer is enormous, and dense fogs prevail, the mean temperature of June does not rise above the freezing-point, being actually  $13^{\circ}\cdot5$  below the normal temperature; while in some parts of Asia under the same latitude, where there is comparatively little ice, the mean temperature of June is as high as  $60^{\circ}$ .

The mean temperature of Van Rensselaer Harbour, in lat.  $78^{\circ} 37'$  N., long.  $70^{\circ} 53'$  W., was accurately determined from hourly observations made day and night over a period of two years by Dr. Kane. It was found to be as follows:—

Winter	. . . . .	$-28^{\circ}\cdot59$
Spring	. . . . .	$-10^{\circ}\cdot59$
Summer	. . . . .	$+33^{\circ}\cdot38$
Autumn	. . . . .	$-4^{\circ}\cdot03$

But although the quantity of heat received from the sun at that latitude ought to have been greater during the summer than in England†, yet nevertheless the temperature is only  $1^{\circ}\cdot38$  above the freezing-point.

The temperature of Port Bowen, lat.  $73^{\circ} 14'$  N., was found to be as follows:—

Winter	. . . . .	$-25^{\circ}\cdot09$
Spring	. . . . .	$-5^{\circ}\cdot77$
Summer	. . . . .	$+34^{\circ}\cdot40$
Autumn	. . . . .	$+10^{\circ}\cdot58$

Here the summer is only  $2^{\circ}\cdot4$  above the freezing-point.

The condition of things in the antarctic regions is even still worse than in the arctic. Captain Sir James Ross, when between lat.  $66^{\circ}$  and  $77^{\circ}\cdot5$ , during the months of January and February 1841, found the mean temperature to be only  $26^{\circ}\cdot5$ ; and there were only two days when it rose even to the freezing-point. When near the ice-barrier on the 8th of February, 1841, a season of the year equivalent to August in England, he had the thermometer at  $12^{\circ}$  at noon; and so rapidly was the young ice forming around the ships, that it was with difficulty that he escaped being frozen in for the winter. “Three days later,” he says, “the thick falling snow prevented our seeing to any distance before us; the waves as they broke over the ships froze as they fell on the decks and rigging, and covered our clothes with a

\* Scoresby’s ‘Arctic Regions,’ vol. i. p. 425.

† See Meech’s memoir “On the Intensity of the Sun’s Heat and Light,” Smithsonian Contributions, vol. ix.

thick coating of ice”\*. On visiting the barrier next year about the same season, he again ran the risk of being frozen in. He states that the surface of the sea presented one unbroken sheet of young ice as far as the eye could discover from the masthead.

Lieutenant Wilkes, of the American Exploring Expedition, says that the temperature they experienced in the antarctic regions surprised him, for they seldom, if ever, had it above  $30^{\circ}$  even at midday.

These extraordinarily low temperatures during summer, which we have just been detailing, were due solely to the presence of snow and ice. In South Georgia, Sandwich Land, and some other places which we have noticed, the summers ought to be about as warm as those of England; yet to such an extent is the air cooled by means of floating ice coming from the antarctic regions, and the rays of the sun enfeebled by the dense fogs which prevail, that there is actually not heat sufficient even in the very middle of summer to melt the snow lying on the sea-beach.

We read with astonishment that a country on the latitude of England should in the very middle of summer be covered with snow down to the sea-shore, and the thermometer seldom rising much above the freezing-point. But we do not consider it so surprising that the summer temperature of the polar regions should be low, for we are accustomed to regard a low temperature as the normal condition of things there. We are, however, mistaken if we suppose that the influence of ice on climate is less marked at the poles than at such places as South Georgia or Sandwich Land.

It is true that a low summer temperature is the normal state of matters in very high latitudes, but it is so only in consequence of the perpetual presence of snow and ice. When we speak of the normal temperature of a place we, of course, as we have already seen, mean the normal temperature under the present condition of things. But were the ice removed from those regions, our present Tables of normal summer temperature would be valueless. These Tables give us the normal June temperature while the ice remains, but they do not afford us the least idea as to what that temperature would be were the ice removed. The mere removal of the ice, all things else remaining the same, would raise the summer temperature enormously. The actual June temperature of Melville Island, for example, is  $37^{\circ}$ , and Port Franklin, Nova Zembla,  $36^{\circ}5$ ; but were the ice removed from the arctic regions, we should then find that the summer temperature of those places would be about as high as that of England. This will be evident from the following considerations:—

\* *Antarctic Regions*, vol. i. p. 240.



The temperature of a place, other things being equal, is proportionate to the quantity of heat received from the sun. If Greenland receives per given surface as much heat from the sun as England, its temperature ought to be as high as that of England. Now from May 10 till August 3, a period of eighty-five days, the quantity of heat received from the sun in consequence of his remaining above the horizon is actually greater at the north pole than at the equator.

Column II. of the following Table, calculated by Mr. Meech\*, represents the quantity of heat received from the sun on the 15th of June at every 10° of latitude. To simplify the Table, I have taken 100 as the unit quantity received at the equator on that day instead of the unit adopted by Mr. Meech:—

	I. Latitude.	II. Quantity of heat.	III. June temperature.
Equator .....	0	100	80°0
	10	111	81·1
	20	118	81·1
	30	123	77·3
	40	125	68·0
	50	125	58·8
	60	123	51·4
	70	127	39·2
	80	133	30·2
North pole...	90	136	27·4

The calculations, of course, are made upon the supposition that the quantity of rays cut off in passing through the atmosphere is the same at the poles as at the equator, which, of course, is not exactly the case. But, notwithstanding the extra loss of solar heat in high latitudes from the greater amount of rays cut off, still, if the temperature of the arctic summers were anything like proportionate to the quantity of heat received from the sun, it ought to be very much higher than it actually is. Column III. represents the actual mean June temperature, according to Prof. Dove, at the corresponding latitudes. A comparison of these two columns will show the very great deficiency of temperature in high latitudes during summer. At the equator, for example, the quantity of heat received is represented by 100 and the temperature 80°; while at the pole the temperature is only 27°·4, although the amount of heat received is 136. This low temperature during summer, from what has been already shown, is due chiefly to the presence of snow and ice. If by some means or

\* See 'Report of Smithsonian Institution' for 1856, p. 330, Smithsonian Contributions, vol. ix.

other we could remove the snow and ice from the arctic regions, they would then enjoy a temperate, if not a hot summer. In Greenland, as we have already seen, even in the very middle of summer snow falls, more or less, nine days out of ten\*; but remove the snow from the northern hemisphere, and a snow-shower in Greenland during summer would be as great a rarity as it would be on the plains of India.

Other things being equal, the quantity of solar heat received at Greenland during summer is considerably greater than in England. Consequently, were it not for snow and ice, it would enjoy as warm a climate during summer as that of England. Conversely, let the polar snow and ice extend to the latitude of England, and the summers of that country would be as cold as those of Greenland. Our summers would then be as cold as our winters are at present, and snow in the very middle of summer would perhaps be as common as rain.

*How the foregoing causes deflect Ocean-currents.*—We shall now see the consequences to which all this leads. A high condition of eccentricity tends, we have seen, to produce an accumulation of snow and ice on the hemisphere whose winters occur in aphelion. The accumulation of snow in turn tends to lower the summer temperature, cut off the sun's rays, and retard the melting of the snow. In short, it tends to produce on that hemisphere a state of glaciation. Exactly opposite effects take place on the other hemisphere, which has its winter in perihelion. There the shortness of the winters and the highness of the temperature, owing to the sun's nearness, tend to prevent the accumulation of snow. The general result is that the one hemisphere is cooled and the other heated. This state of things now brings into play the agencies which lead to the deflection of the Gulf-stream and other great ocean-currents.

Owing to the great difference between the temperature of the equator and the poles, there is a constant flow of air from the poles to the equator. It is to this that the trade-winds owe their existence. Now as the strength of these winds will, as a general rule, depend upon the difference of temperature that may exist between the equator and higher latitudes, it follows that the trades on the cold hemisphere will be stronger than those on the warm. When the polar and temperate regions of the one hemisphere are covered to a large extent with snow and ice, the air, as we have just seen, is kept almost at the freezing-point during both summer and winter. The trades on that hemisphere will of necessity be exceedingly powerful; while on the other hemisphere, where there is comparatively little snow and ice and the air is warm, the trades will, as a consequence, be

\* Arctic Regions, vol. i. p. 425.

weak. Suppose now the northern hemisphere to be the cold one. The north-east trade-winds of this hemisphere will far exceed in strength the south-east trade-winds of the southern hemisphere. The *median line* between the trades will consequently lie to a very considerable distance to the south of the equator. We have a good example of this at the present day. The difference of temperature between the two hemispheres at present is but trifling to what it would be in the case under consideration ; yet we find that the south-east trades of the Atlantic blow with greater force than the north-east trades, and the result is that the south-east trades sometimes extend to  $10^{\circ}$  or  $15^{\circ}$  N. lat., whereas the north-east trades seldom blow south of the equator. The effect of the northern trades blowing across the equator to a great distance will be to impel the warm water of the tropics over into the Southern Ocean. But this is not all ; not only would the median line of the trades be shifted southwards, but the great equatorial currents of the globe would also be shifted southwards.

Let us now consider how this would affect the Gulf-stream. The South-American continent is shaped somewhat in the form of a triangle, with one of its angular corners, called Cape St. Roque, pointing eastwards. The equatorial current of the Atlantic impinges against this corner ; but as the greater portion of the current lies a little to the north of the corner, it flows westward into the Gulf of Mexico and forms the Gulf-stream. A considerable portion of the water, however, strikes the land to the south of the Cape and is deflected along the shores of Brazil into the Southern Ocean, forming what is known as the Brazilian current.

Now it is perfectly obvious that the shifting of the equatorial current of the Atlantic only a few degrees to the south of its present position—a thing which would certainly take place under the conditions which we have been detailing—would turn the entire current into the Brazilian branch, and instead of flowing chiefly into the Gulf of Mexico as at present, it would all flow into the Southern Ocean, and the Gulf-stream would consequently be stopped. The stoppage of the Gulf-stream, combined with all those causes which we have just been considering, would place Europe under a glacial condition, while at the same time the temperature of the Southern Ocean would, in consequence of the enormous quantity of warm water received, have its temperature (already high from other causes) raised enormously.

And what holds true in regard to the currents of the Atlantic holds also true, though perhaps not to the same extent, of the currents of the Pacific.

But there is still another cause which must be noticed :—A

strong undercurrent of air *from* the north implies an equally strong upper current *to* the north. Now if the effect of the undercurrent would be to impel the warm water at the equator to the south, the effect of the upper current would be to carry the aqueous vapour formed at the equator to the north; the upper current, on reaching the snow and ice of temperate regions, would deposit its moisture in the form of snow; so that it is probable that, notwithstanding the great cold of the glacial epoch, the quantity of snow falling in the northern regions would be enormous. This would be particularly the case during summer, when the earth would be in the perihelion and the heat at the equator great. The equator would be the furnace where evaporation would take place, and the snow and ice of temperate regions would act as a condenser.

The direct effect of eccentricity is to produce on one of the hemispheres a long and cold winter. This alone would not lead to a condition of things so severe as that which we know prevailed during the glacial epoch. But the snow and ice thus produced would bring into operation, as we have seen, a host of physical agencies whose combined efforts would be perfectly sufficient to do this.

*A remarkable Circumstance regarding those Causes which lead to Secular Changes of Climate.*—There is one remarkable circumstance connected with those physical causes which deserves special notice. They not only all lead to one result, viz. an accumulation of snow and ice, but they mutually react on one another. It is quite a common thing in physics for the effect to react on the cause. In electricity and magnetism, for example, cause and effect in almost every case mutually act and react upon each other. But it is usually, if not universally, the case that the reaction of the effect tends to weaken the cause. The weakening influences of this reaction tend to impose a limit on the efficiency of the cause. But, strange to say, in regard to the physical causes concerned in the bringing about of the glacial condition of climate, cause and effect mutually reacted so as to strengthen each other. And this circumstance had a great deal to do with the extraordinary results produced.

We have seen that the accumulation of snow and ice on the ground resulting from the long and cold winters tended to cool the air and produce fogs which cut off the sun's rays. The rays thus cut off diminished the melting-power of the sun, and so increased the accumulation. As the snow and ice continued to accumulate, more and more of the rays were cut off; and on the other hand, as the rays continued to be cut off, the *rate* of accumulation increased, because the quantity of snow and ice melted became thus annually less and less.



Again, during the long and dreary winters of the glacial epoch the earth would be radiating off its heat into space. Had the heat thus lost simply gone to lower the temperature, the lowering of the temperature would have tended to diminish the rate of loss; but it was the formation of snow and ice more than the lowering of temperature that was the result.

And, again, the formation of snow and ice facilitated the rate at which the earth lost its heat; and on the other hand, the more rapidly the earth parted with its heat, the more rapidly were the snow and ice formed.

Further, as the snow and ice accumulated on the one hemisphere, they at the same time continued to diminish on the other. This tended to increase the strength of the trade-winds on the cold hemisphere, and to weaken those on the warm. The effect of this on ocean-currents would be to impel the warm water of the tropics more to the warm hemisphere than to the cold. Suppose the northern hemisphere to be the cold one, then as the snow and ice began gradually to accumulate on it, the ocean-currents of that hemisphere would begin to decrease in volume, while those on the southern, or warm hemisphere, would *pari passu* increase. This withdrawal of heat from the northern hemisphere would tend, of course, to lower the temperature of that hemisphere and thus favour the accumulation of snow and ice. The more the snow and ice accumulated, the less, on this account, would grow the ocean-currents; and on the other hand, the less the ocean-currents became, the greater would become the accumulation of snow and ice,—the two effects mutually strengthening each other.

The same must have held true in regard to aërial currents. The more the polar and temperate regions became covered with snow and ice, the stronger would become the trades and anti-trades of the hemisphere; and the stronger those winds became, the greater would be the amount of moisture transferred from the tropical regions by the anti-trades to the temperate regions; and on the other hand, the more moisture those winds brought to temperate regions, the greater would be the quantity of snow produced.

The same process of mutual action and reaction would take place among the agencies in operation on the warm hemisphere, only the result produced would be the diametrically opposite of that produced in the cold hemisphere. On this warm hemisphere action and reaction would tend to raise the mean temperature and diminish the quantity of snow and ice existing in temperate and polar regions.

Had it been possible for each of those various physical agents which we have been considering to produce its direct effects

without influencing the other agents, or being influenced by them, its real efficiency in bringing about either the glacial condition of climate or the warm condition of climate, as the case may have been, would not have been so great.

The primary cause that set all those various physical agencies in operation which brought about the glacial epoch, was a high state of eccentricity of the earth's orbit. When the eccentricity is at a high value, snow and ice begin to accumulate, from the increasing length and coldness of the winter, on that hemisphere whose winter solstice is approaching toward the aphelion. The accumulating snow then begins to bring into operation all the various agencies which we have been describing; and, as we have just seen, these, when once in full operation, mutually aid one another. As the eccentricity increases century by century, the temperate regions become more and more covered with snow and ice, first on account of the continued increase in the coldness and length of the winters, and secondly, and chiefly, from the continued increase in the potency of those physical agents which have been called into operation. This glacial state of things goes on at an increasing rate, and reaches a maximum when the solstice point arrives at the aphelion. After the solstice passes the aphelion, a contrary process commences. The snow and ice gradually begin to diminish on the cold hemisphere and to make their appearance on the other hemisphere. The glaciated hemisphere begins to turn warmer and the warm hemisphere colder, and this continues to go on for a period of ten or twelve thousand years, until the winter solstice reaches the perihelion. By this time the conditions of the two hemispheres are reversed; the formerly glaciated hemisphere has now become the warm one, and the warm hemisphere the glaciated. The transference of the ice from the one hemisphere to the other continues so long as the eccentricity remains at a high value.

*The Mean Temperature of the whole Earth should be greater in Aphelion than in Perihelion.*—When the eccentricity becomes reduced to about its present value, its influence on climate is but little felt. It is, however, probable that the present extensions of ice on the southern hemisphere may, to a considerable extent, be the result of eccentricity. The difference in the climatic conditions of the two hemispheres is just what should be according to theory:—(1) The mean temperature of that hemisphere is less than that of the northern. (2) The winters of the southern hemisphere are colder than those of the northern. (3) The summers, though occurring in perihelion, are also comparatively cold; this, as we have seen, is what ought to be according to theory. (4) The mean temperature of the whole

earth is greater in June, when the earth is in aphelion, than in December, when it is in perihelion. This, I venture to affirm, is also what ought to follow according to theory, although this very fact has been adduced as a proof that eccentricity has at present but little effect on the climatic condition of our globe.

That the mean temperature of the whole earth would, during the glacial epoch, be greater when the earth was in aphelion than when in perihelion will, I think, be apparent from the following considerations:—When the earth was in the perihelion, the sun would be over the hemisphere nearly covered with snow and ice. The great strength of the sun's rays would in this case have little effect in raising the temperature; it would be spent in melting the snow and ice. But when the earth was in the aphelion, the sun would be over the hemisphere comparatively free, or perhaps wholly free, from snow and ice. Consequently, though the intensity of the sun's rays would be less than when the earth was in perihelion, still it ought to produce a higher temperature, because it would be chiefly employed in heating the ground, and not consumed in melting snow and ice.

The present difference between the climatic condition of the two hemispheres is exactly what it ought to be on the supposition that it is produced by eccentricity. From what has already been stated, I presume that the low temperature of the southern hemisphere cannot be referred to the mere distribution of sea and land, unless we admit that an enormous amount of heat is constantly being transferred by means of ocean-currents from the southern hemisphere to the northern. For, as we have seen, the physical properties of water, *statically* considered, ought to produce a higher *mean* temperature than that of land. And if it be admitted that the present lower mean temperature of the southern hemisphere is owing to the transference of heat by ocean-currents, the question arises, to what extent is this transference due to eccentricity?

*An objection answered.*—A very common misconception regarding the physical cause of the motion of ocean-currents will no doubt suggest to the minds of some readers an objection to the conclusion arrived at in reference to the displacement of the currents from the cold to the warm hemisphere. In order to meet this objection, it will be necessary to consider at some length the misconception to which I refer.

[To be continued.]

XXV. *On Mica-combinations.*

By Professor REUSCH, of Tübingen\*.

1. **W**HEN one superimposes on each other an even number of thin films of biaxial mica in such a manner that the principal sections cross alternately at an angle of  $90^\circ$ , one obtains, even with a moderate number of such crossings, an arrangement the behaviour of which is approximately that of a uniaxial crystal. In the case of the principal sections of the mica coinciding with the crossed planes of polarization, the imitation is perfect; on turning the arrangement, however, in its plane, the coloured rings remain, but the arms of the black cross become bright, and after a rotation of  $45^\circ$  there is left a short-armed cross in the innermost ring only. Nörremberg, to whom this experiment is due, was led thereto by the well-known labours of Sénarmont upon mica and Rochelle salt. Quenstedt's 'Mineralogy' (second edition, p. 239) is, to the best of my knowledge, the only work in which reference is made thereto. It is nowise out of place to remark that Mr. Steeg, optician at Homburg, furnishes these arrangements executed with superior perfection. (See No. 77 of his Catalogue for 1867.)

2. I have recently occupied myself with certain novel mica-combinations, the object I had in view being the imitation of the behaviour of right-handed or of left-handed uniaxial crystals. I will first of all, with the aid of the figures below, give a description of these combinations.

Fig. 1.

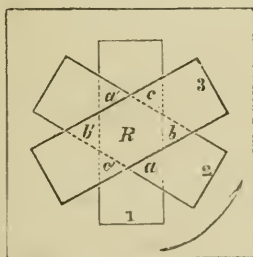
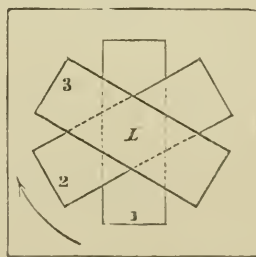


Fig. 2.



Upon two glass plates, pieces of cardboard were cemented wherein three rectangular apertures, crossing each other at an angle of  $60^\circ$ , had been previously cut for the reception of the oblong mica films. The films themselves were derived from a biaxial mica of above  $80^\circ$  between the axes; they were as thin, and as uniform in their thickness, as possible, and, in all, the prin-

\* Translated from the Monthly Report of the Berlin Academy of Sciences for July 1869, and kindly communicated by W. G. Lettsom, Esq.



cipal section coincided with the longer dimension. Let us suppose we have prepared forty-eight plates. Half of this number is to be used to pile up a flight of steps, so to speak, ascending in the order 1, 2, 3 from left to right, as shown in fig. 1; the other half is to be employed to build up a flight of steps ascending from right to left, as shown in fig. 2. Prior to laying an additional film on the one already in position, a small drop of thick copal varnish is to be laid upon the latter, and the fresh film is to be pressed down thereon lightly. By this means there are prepared two arrangements or systems, the plates whereof in the one marked R (fig. 1), to an observer who looks at the stairs from the side, ascend towards the right, and in the one marked L (fig. 2) ascend towards the left. Botanists, who in order to determine the direction of a spiral imagine themselves placed in its axis, would, it is true, and perhaps with more propriety, look upon the system marked R as a left-handed one, and the one marked L as being right-handed; in what follows, however, I shall abide by what is technically and in the language of common life held to be a right-handed and a left-handed screw.

Now, in the central regular hexagonal portion where the superposition occurs, the systems R and L behave very nearly like a right-handed or left-handed rock-crystal. Even with not more than from four to six turns of mica not excessively thin, the direction of the rotation may be ascertained upon turning the upper Nicol's prism round: in Nörrenberg's polarizing microscope with a wide field one observes the system of rings with a bluish central cross; and on employing both arrangements together superposed, very satisfactory indications of Airy's spirals are manifested.

Precisely similar results are arrived at by the use of two arrangements in which four systems of plates are stacked up, forming a right-handed and left-handed series crossed at an angle of  $45^\circ$ .

The arrangements that I constructed in the first instance consisted in part of films not very thin, and the thickness of which was not completely uniform; in part, too, the number of turns was but limited (from three to six); and I therefore addressed myself to Mr. Steeg, from whom I speedily received two pairs of systems crossed at an angle of  $60^\circ$ . They are surprisingly large and are executed with extraordinary perfection, and exhibit with brilliancy the change of colour on the upper Nicol being turned. One pair consists, in each combination, of thirty films of  $\frac{1}{8}\lambda$ , the other of no less than thirty-six films of even a less thickness. The first pair gives for red light a rotation of  $150^\circ$ , corresponding to a thickness of quartz of about 8 millims. As a proof of the great precision and skill with which Mr. Steeg manipulates

mica, I may state that the seventy-two films of the second pair, 12 millims. in width and 30 millims. long, were all cut from the same plate. From a thin plate ( $\frac{1}{8}\lambda$ ), for which I am indebted to Mr. Steeg's special kindness, I have subsequently prepared systems with four films crossed at  $45^\circ$  which showed Airy's spiral, whereas my original preparations exhibited, it is true, a slight change of colour when the Nicol was turned, but gave when superposed a confused image of the rings.

If a mica-combination of this construction is turned round in its plane in converging light, the planes of polarization being crossed, the rings indeed remain, but the arms of the black cross undergo modifications; and we observe that at the ends of the diameters of the innermost ring, those diameters being situated in the planes of polarization, dark patches enter and withdraw. In like manner, in parallel light, the colour undergoes slight changes on turning the Nicol round, but the modification takes place rather in the intensity than in the tone of the colour.

I have found that these properties of a mica-combination may be imparted to a quartz by inserting both above and below it an eighth-of-undulation mica film, the principal sections of those films being crossed at right angles. The mica-combinations are consequently to be looked upon as elliptically right and left polarizing media, which approximate all the closer to quartz the thinner the films are and the greater the number of the turns is.

In like manner, the modifications of the black cross in Nörremberg's combination (treated of in paragraph 1) may be produced with a plate of calc-spar cut at right angles to the axis when combined in the manner described with eighth-of-undulation films, the whole arrangement being turned round in its plane. Hence we see in the mica-combination the phenomenon as though a non-rotating uniaxial crystal were elliptically polarized and analyzed.

3. In forming the layers of the films at an angle of  $60^\circ$ , there are produced on the sides of the central hexagon equilateral triangles in which only two systems of films alternately cross at  $60^\circ$ . It is easy to convince oneself that in these triangles, according to their position, one has to do with elliptical right or left rotation. I was thereby led to investigate, as a preliminary step, the conduct of two plates of any thickness whatever, the principal sections of which were crossed at an angle differing from  $90^\circ$ . Speaking generally, such a combination gives right or left elliptically polarized light; that is to say, upon turning the upper Nicol, one arrives at determining the direction of the rotation, but upon turning the combination in its plane, the intensity of the colour is changed, and also its tint as well. The experiment is successful, not only with any two mica or selenite plates, but with

a combination of mica and selenite, provided only that their colours do not belong to too high an order.

Fig. 3.

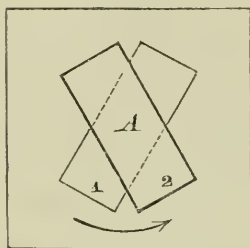
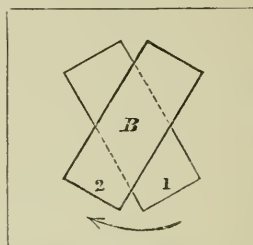


Fig. 4.



In what follows I occupy myself solely with *mica plates of equal thickness*. Two such plates, wherein, as before, the principal section is to correspond with the longer dimension, can be combined to form either a right-ascending stair, A (fig. 3), or a left-ascending one, B (fig. 4). Two such stairs possess in any case opposite optical rotation, but the direction of the rotation is determined by the thickness of the plates. If, for instance, the plates give green of the second order, then the right-ascending stair A gives also right-handed rotation; with plates that give yellow of the first order the contrary is the case. The angle of the principal sections is without effect with regard to the direction of the rotation; it must, however, differ to a suitable amount from  $0^\circ$  and  $90^\circ$ . For instance, if two stairs formed of plates crossing at an angle of  $60^\circ$  (A and B) are placed on each other with parallel principal sections, there always remains a rotation in the direction of the uppermost stair. When the stairs are crossed at right angles, no kind of action is produced in the central superposed portion, and this whatever the azimuth of the combined stair may be. The two arms of the cross arising from the superposition of plates of various stairs are possessed of opposite rotation.

A case of higher interest, however, is presented when many equal stairs consisting of very thin films of mica are stacked up to form a right- or left-ascending combination. In that case the rotation of the right-ascending combination is right-handed, and that of the left-ascending combination is left-handed. We find therein, therefore, at once the explanation of the triangles *a*, *b*, *c* of the combination crossing at  $60^\circ$ , as represented in fig. 1. The first two belong to a right-handed combination, the last to a left-handed one. The triangles *a'*, *b'*, *c'* act, of course, as opposite corresponding ones. By the same rule are determined the directions of the rotation in the external points of the star of four systems of plates in a combination crossing at  $45^\circ$ .

These stacked-up combinations, or piles, exhibit also another peculiarity: in converging light there is seen through the superposed portion a biaxial system, the principal section of which bisects the acute angle of the principal sections of the mica films, and the angle subtended by the axis of which is less than that of the mica employed. The black hyperbolas, however, appear only when the principal section of the mica-combination forms an angle of  $45^\circ$  with the planes of polarization; in the case of its coinciding with either the one or the other, the innermost rings exhibit merely black spots.

The effect of a pile thus built up may be imitated with something like approximation by means of a thick plate of mica so inserted between two eighth-of-undulation plates, whose axes are crossed at a right angle, that the principal section of the mica plate makes an angle of  $45^\circ$  with them; and this combination has a different direction of rotation in parallel light according as the principal section of the plate bisects the one or the other pair of the vertical quadrants formed by the principal sections of the eighth-of-undulation plates. This reminds one of an arrangement described by Dr. John Müller, in the seventh edition of his '*Natural Philosophy*' (vol. i. p. 906), wherein, by means of an analogous combination of a selenite plate with two quarter-undulation mica plates, the effect of quartz is imitated, at least in parallel light.

4. The optical effects of the mica-combinations above described admit of mathematical treatment; and it is to be presumed the phenomena in parallel light will not present anything like the difficulty which those will offer when converging light is used. Perhaps some competent calculator may be induced to occupy himself with this not altogether thankless problem.

The future will teach us whether these combinations are calculated to afford us an explanation, or at least hints, respecting the connexion of particles endowed with circular polarization. At present in this respect I can only adduce a feeble analogy and a few suggestions, to which I would direct the attention of those who are learned in such matters.

In a former communication upon the so-called lamellar polarization of alum (see General Meeting of the Academy, July 11, 1867), I have endeavoured to establish that what we have to do with here is a feeble double refraction due to internal tensions, which must be conceived to be so acting in the planes of the octahedron that the optical elasticity is equal in those planes in all directions, but less than at right angles thereto. I have further shown how the effect of an optically active octahedron of alum, or of a preparation therefrom in the direction of two parallel planes of the cube, may be perfectly imitated in the four



distinct quadrants by four thin films of mica. In this mica arrangement, however, there occur no superpositions, whereas in the new arrangements it is precisely upon superpositions that the entire matter is founded. Hence there arises conversely the question with respect to that crystalline structure which corresponds to a mica-combination with superpositions.

An idea that naturally suggests itself is as follows—namely, that in the ideal active octahedron of alum the planes of tension parallel to any plane of the octahedron extend only up to the three rectangular axial planes; but it is also conceivable that the derangement brought about in the original structure of a crystal by means of internal tensions and contractions may be referred to one or to several planes of tension inclined to the direction of the transmitted light, and which always traverse the entire crystal in a constant direction. Now we know, it is true, but very little respecting the normal structure of crystals, and consequently even much less with regard to the derangements thereof actually met with; so that if one chooses to take the matter up at all, one finds oneself confined for the present to a few instinctive conjectures.

In the regular system, in the octahedron of the active alum octahedron, we meet with the case of individual planes of tension that do not pass totally through. The optical phenomena must become more complicated when other planes, those for instance of the leucitoid, occur as distinct planes of tension (leucite, analcime?). The circular polarization of chlorate of soda, discovered by Marbach, is possibly the result of tensions in the direction of the planes of the dodecahedron, combined with secondary tensions in the direction of those of the right or left tetrahedron; the mica-combination of four systems of plates at an angle of  $45^\circ$ , when the effect of the separate turns is extremely feeble, affords perhaps a representation thereof.

The same mica-combination corresponds perhaps also to the case of circular polarization in the tetragonal system. Of the four systems of plates, 1 and 3 would to a certain extent represent the structure of a uniaxial non-rotating crystal; the systems of plates 2 and 4 would be the equivalent of tensions traversing the crystal in totality in the direction of the planes of the right or left hemioctahedron. The occurrence of a single plane of tension inclined to the axis, or the unequal intensity of the individual tensions, would be manifested by the biaxial character of the optical phenomena (the dislocation of the black cross in beryl, in ferrocyanide of potassium, &c.).

Circular polarization in the rhombohedral system is probably the result of three planes of tension inclined at equal angles to the axis and traversing the entire crystal, these planes agreeing

perhaps with those of the one or the other of the two fellow hemiscalenohedrons. The idea of the possibility of internal tensions of this nature does not suggest itself respecting any substance so readily as with regard to silica. If the three tensions are perfectly equivalent, we shall obtain the normal right- or left-rotating effect of quartz; if all the three tensions are wanting, or when they are balanced reciprocally, there will remain the simple uniaxial effect without rotation, as we have it in many amethysts at some portions. There yet remains the possibility that, according to circumstances, those three tensions are of unequal intensity or are reduced to two; and we should then have the biaxial elliptical right- or left-rotating polarization met with in such a marked manner in various quartzes, and as it is observed in the built-up piles above described, or in arrangements wherein one of the three systems of plates consists designedly of somewhat thicker or thinner mica.

With regard to the various phenomena in quartz and amethyst, I beg leave to refer to the copious and well-arranged observations contained in Dove's *Farbenlehre* (pp. 247–260).

In conclusion, I must add that an acquaintance with the effects of mica-combinations may possibly be of some value also for the due appreciation of certain phenomena in mica itself. The transformations of the system of rings in twins, as well as also the frequent very considerable changes in the angle of the optic axes in the same specimen, are rendered to a certain extent intelligible if we assume the existence of regular penetration and intersection on the part of different individuals. For my acquaintance with these phenomena I am principally indebted to the communications I have received from G. Rose, and to the numerous presents of mica which he has kindly made me, and the possession of which originally gave rise to my formation of the new mica-combinations.

Tübingen, July 1869.

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XXVI. On Criticoids. By Sir JAMES COCKLE, F.R.S., the Chief Justice of Queensland, and President of the Queensland Philosophical Society\*.

1. **I**N continuing to use the term critical function, instead of seminvariant peninvariant or leading coefficient or source of covariant, I do so, not from any predilection, but to avoid words which might lead to the inference that the analogies, here pointed out, between algebra and the differential calculus, are susceptible of a greater extension than I am now prepared to

\* Communicated by the Rev. Robert Harley, F.R.S.

show that they admit of. By a critical function of the roots or coefficients of an algebraical equation I mean a function which remains unaltered when each of the roots is increased or diminished by any (the same) quantity whatever, or which remains unaltered when  $x+h$  is substituted for  $x$  in a given equation in  $x$ . The forms of these functions\* are well known. The researches of Spence in connexion with them are not, however, so well known†. As seminvariants or as invariants, critical functions play a conspicuous part in the grand theory of a later period, the theory of covariants.

2. A criticoid stands in the same relation to a factorially transformed linear differential equation that a critical function fulfils with respect to a linearly transformed algebraical equation. I retain the word quantoid to signify the sinister of a linear differential equation whereof the dexter is zero; and that a general quantoid has a quadricriticoid and a cubicriticoid I have already‡ shown. Illustrations of the use of the quadricriticoid will be found in places mentioned below§. The object of this paper is, not to apply the theory, but to show the existence of criticoids of higher degrees and actually to deduce expressions for quarticriticoids.

3. In attempting to obtain quarticriticoids various courses presented themselves. First, I deduced them, as in the lower cases, by elimination. This course has its advantages. Its disadvantages are want of directness and of generality, and a failure of proof of the existence of criticoids other than those actually obtained or deducible therefrom by combination. Secondly, in conformity with the analogies of the theory of algebraical equations, I assumed that the coefficients of a quantoid whereof the second term is made to vanish are criticoids. This course, though not devoid of advantage, labours under the same disadvantages as the first, with the additional one that the results require testing. Thirdly, I followed a new course which gives by a direct process results perfectly general and makes manifest the exist-

\* Some information respecting them will be found in my papers respectively entitled "On Critical and Spencian Functions, with Remarks upon Spence's Theory" (Quarterly Journal of Mathematics, vol. iv. pp. 97-111), and "On the General Forms of Critical Functions" (ibid. pp. 265-270).

† Of Spence's (posthumous) 'Outlines,' &c. it is said that there were only eighty copies printed.

‡ "Correlations of Analysis" (Phil. Mag. S. 4. vol. xxiv. pp. 531-534). The same results are exhibited under a slightly different form in my paper "On Quantoids" (Phil. Mag. for November 1865).

§ See my "Notes on the Differential Calculus," published in the 'Messenger of Mathematics' (vol. iii. pp. 42-50, and pp. 247-256). In a paper "On the Integration of Differential Equations" (Mathematical Reprint of the Educational Times, vol. ix. pp. 105-112) I have to some extent employed the quadricriticoid.

ence of criticoids of all degrees. Lastly, I did not attempt to employ an operative symbol; for I am not satisfied with the theory of the symbol as it at present stands\*; and even if such

\* See papers "On Differential Covariants" (Phil. Mag. for March 1864) and "On the Operating Symbol of Differential Covariants" (ibid. for September 1864), in which I used or implied the use of the term differential covariants in reference to certain functions which in the paper "On Quantoids" (ibid. for November 1865) I called covaroids. But such functions are analogous, not to covariants, but to a more restricted form of algebraical function. From the two forms

$$(1, a_1, a_2, \dots a_n \text{X} x, 1)^n, \quad (1, A_1, A_2, \dots A_n \text{X} x, 1)^n$$

we may, by making  $n = 1, 2, 3, \&c.$  successively, derive two sets of quantics. If the linear substitution of  $x+h$  for  $x$  converts the last quantic of one set into the last quantic of the other, it will convert every quantic of one set into the corresponding quantic of the other. Let us then call all the quantics of a set uniform. Then a function of uniform functions is uniform, for the linear transformation will transform it into a corresponding function. If a uniform function, using the term in its general sense, be porismatic with respect to  $x$  (that is, if  $x$  disappears from it), the uniform function is critical. If  $x$  disappears partly, but not wholly, the terms in which it occurred being replaced by critical functions, the uniform function so modified may be termed a semicritical function. The differential functions which I have called covaroids, but which ought rather to be called semicriticoids, are the analogues of these algebraical semicritical functions, which latter may be defined as functions of critical and of uniform functions. If we divide each side of equation (8) of the text by  $u$ , either side of the equation so divided may be regarded as the form of a set of uniform differential functions, and any function of such functions will be uniform under factorial substitution. If  $u$  disappears from a uniform function, such function is a criticoid. If  $u$  disappears partly, but not wholly, the disappearing terms being replaced by criticoids, we have a semicriticoid, which may be defined as being a function of criticoids and of uniform differential func-

tions. In my paper "On Quantoids," the expression  $\frac{d^p y_q}{dx^p}$  should be replaced by  $\delta^p y_q$ ; and, in conformity with the terminology of this footnote, I should now paraphrase art. 3 of that paper to some extent by saying that  $y_m, y_n$ , and  $\delta^p y_q$  are uniform, and that any function of any number of such expressions and of criticoids is a semicriticoid. The paper "On Differential Covariants" suggests two remarks.

First. If we study the quantoid  $y_n$  under the homogeneous form

$$y_n = (a_0, a_1, \dots a_n \text{X} \frac{d}{dx}, 1)^n y,$$

and call its basic criticoids  $k_2, k_3, \&c.$ , and those of the transformed quantoid  $K_2, K_3, \&c.$ , we shall in general have

$$u^m k_m = K_m,$$

wherein, for instance,

$$k_2 = a_0 a_2 - a_1^2 - \left( a_0 \frac{da_1}{dx} - a_1 \frac{da_0}{dx} \right), \quad k_2 = \&c.,$$

and  $K_m$  is the same function of  $A_0, A_1, \&c.$ , the coefficients of the transformed quantoid that  $k_m$  is of  $a_0, a_1, \&c.$  the corresponding respective co-



theory can be completed in a satisfactory manner, the operator would, it seems to me, affect separately the parts involving and the parts free from differentiation, and so render a further test requisite.

4. It will be convenient to use the following notation. Put

$$\frac{1}{u} \frac{d^r u}{dx^r} = u_r, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

then we have, identically,

$$u_2 - u_1^2 - \frac{du_1}{dx} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$u_3 - 3u_1 u_2 + 2u_1^3 - \frac{d^2 u_1}{dx^2} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$u_4 - 4u_1 u_3 - 3u_2^2 + 12u_1^2 u_2 - 6u_1^4 - \frac{d^3 u_1}{dx^3} = 0. \quad (4)$$

5. If the quantoid

$$a_0(1, a_1, a_2, \dots a_n) \left( \frac{d}{dx}, 1 \right)^n y = y_n \quad . \quad . \quad . \quad (5)$$

be transformed by the factorial substitution

$$y = uY \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

into the quantoid

$$A_0(1, A_1, A_2, \dots A_n) \left( \frac{d}{dx}, 1 \right)^n Y = Y_n, \quad . \quad . \quad . \quad (7)$$

$Y_n$  being equal to  $y_n$ , then generally

$$(1, a_1, a_2, \dots a_m) \left( \frac{d}{dx}, 1 \right)^m u = A_m u. \quad . \quad . \quad . \quad (8)$$

efficients of the original quantoid. In this case the criticoids are not identical after transformation, but one is a factor of the other.

Secondly. If we study the divided quantoid  $(y_n) \div y$ , wherein

$$(y_n) = (1, a_1, a_2, \dots a_n) \left( \frac{d}{dx}, 1 \right)^n y,$$

we obtain results very closely conforming to some of the analogies of algebraical notation; for, adopting the notation signified by equation (1) of the text, we have, applying that notation on the dexter only,

$$\frac{(y_n)}{y} = (y + a)^n,$$

provided that after expanding the dexter we change exponents into suffixes. And the transformation (6) of the text will be indicated as to its results by

$$\frac{(Y_n)}{Y} = (Y + A)^n,$$

on the dexter of which the like change is to be made, and for which equation (13) of the text is supposed to hold. The use of the divided quantoid facilitates the definition of a semicriticoid.

Hence, availing ourselves of the notation of art. 4, we have the system

$$u_1 + a_1 = A_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$u_2 + 2a_1 u_1 + a_2 = A_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

$$u_3 + 3a_1u_2 + 3a_2u_1 + a_3 = A_3. \quad . \quad . \quad . \quad . \quad (11)$$

$$u_4 + 4a_1u_3 + 6a_2u_2 + 4a_3u_1 + a_4 = A_4, \quad . \quad . \quad . \quad (12)$$

whereof each member is capable of being represented by

$$(u+a)^m = A_m, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

provided that after expansion we change exponents into suffixes.

6. All critical functions, under which term both invariants and seminvariants may be included, are capable of being exhibited as rational and entire functions of those critical functions which I have denominated primary. In order to form a quarticriticoid, I take a function corresponding to the quadrinvariant of a quartic, and I find that, on substitution and multiplication, many of the terms cancel one another, and that on reduction we have the following result,

$$A_4 - 4A_1A_3 + 3A_2^2 = a_4 - 4a_1a_3 + 3a_2^2 + 12(a_2 - a_1^2)(u_2 - u_1^2) + 3u_2^2 - 4u_1u_3 + u_4 \quad (14)$$

which, in virtue of (4), becomes

$$A_4 - 4A_1A_3 + 3A_2^2 = a_4 - 4a_1a_3 + 3a_2^2 + 6u_2^2 - 12u_1^2u_2 + 6u_1^4 + \frac{d^3u_1}{dx^3} + 12(a_2 - a_1^2)(u_2 - u_1^2), \quad (15)$$

whence, on reduction, we have

$$A_4 - 4A_1A_3 + 3A_2^2 = a_4 - 4a_1a_3 + 3a_2^2 + \frac{d^3u_1}{dx^3} + 12(a_2 - a_1^2)(u_2 - u_1^2) + 6(u_2 - u_1^2)^2. \quad (16)$$

Now, applying (2) and (9) successively, we find

$$\begin{aligned} 12(a_2 - a_1^2)(u_2 - u_1^2) + 6(u_2 - u_1^2)^2 &= 12(a_2 - a_1^2) \frac{du_1}{dx} + 6\left(\frac{du_1}{dx}\right)^2 \\ &= 12(a_2 - a_1^2)\left(\frac{d\Lambda_1}{dx} - \frac{da_1}{dx}\right) + 6\left(\frac{d\Lambda_1}{dx} - \frac{da_1}{dx}\right)^2 \\ &= 6\left\{a_2 - a_1^2 - \frac{da_1}{dx} + \frac{d\Lambda_1}{dx}\right\}\left(\frac{d\Lambda_1}{dx} - \frac{da_1}{dx}\right) \\ &\quad + 6(a_2 - a_1^2)\left(\frac{d\Lambda_1}{dx} - \frac{da_1}{dx}\right). \quad \dots \dots \dots (17) \end{aligned}$$

But, by the characteristic property of the quadricriticoid,

$$\frac{dA_1}{dx} - \frac{da_1}{dx} = A_2 - A_1^2 - (a_2 - a_1^2); \quad . \quad . \quad . \quad . \quad (18)$$

hence we may write, and reduce, (17) as follows:

$$\begin{aligned} & 12(a_2 - a_1^2)(u_2 - u_1^2) + 6(u_2 - u_1^2)^2 \\ &= 6(A_2 - A_1^2) \left( \frac{dA_1}{dx} - \frac{da_1}{dx} \right) + 6(a_2 - a_1^2) \left( \frac{dA_1}{dx} - \frac{da_1}{dx} \right) \\ &= 6 \{ A_2 - A_1^2 + (a_2 - a_1^2) \} \{ A_2 - A_1^2 - (a_2 - a_1^2) \} \\ &= 6 \{ (A_2 - A_1^2)^2 - (a_2 - a_1^2)^2 \}. \quad . \quad . \quad . \quad . \quad (19) \end{aligned}$$

We also have, from (9),

$$\frac{d^3 u_1}{dx^3} = \frac{d^3 A_1}{dx^3} - \frac{d^3 a_1}{dx^3}. \quad . \quad . \quad . \quad . \quad . \quad (20)$$

Consequently making in (16) the substitutions indicated by (19) and (20), we have

$$\begin{aligned} A_4 - 4A_1A_3 + 3A_2^2 &= a_4 - 4a_1a_3 + 3a_2^2 + \frac{d^3 A_1}{dx^3} - \frac{d^3 a_1}{dx^3} \\ &\quad + 6(A_2 - A_1^2)^2 - 6(a_2 - a_1^2)^2, \quad . \quad . \quad . \quad . \quad (21) \end{aligned}$$

which, after transpositions, becomes

$$\begin{aligned} & A_4 - 4A_1A_3 + 3A_2^2 - 6(A_2 - A_1^2)^2 - \frac{d^3 A_1}{dx^3} \\ &= a_4 - 4a_1a_3 + 3a_2^2 - 6(a_2 - a_1^2)^2 - \frac{d^3 a_1}{dx^3}. \quad . \quad . \quad (22) \end{aligned}$$

7. We may write (22) thus,

$$\begin{aligned} & A_4 - 4A_1A_3 - 3A_2^2 + 12A_1^2A_2 - 6A_1^4 - \frac{d^3 A_1}{dx^3} \\ &= a_4 - 4a_1a_3 - 3a_2^2 + 12a_1^2a_2 - 6a_1^4 - \frac{d^3 a_1}{dx^3}; \quad . \quad (23) \end{aligned}$$

and we see that the expression on the dexter of (22) or (23) remains unchanged after the factorial substitution denoted by (6), and consequently that such expression (that is to say,

$$a_4 - 4a_1a_3 - 3a_2^2 + 12a_1^2a_2 - 6a_1^4 - \frac{d^3 a_1}{dx^3},$$

which I shall represent by  $\nabla$ ) is a quarticriticoid. If we put





spectively become

$$-\frac{da_1}{dx} + a_1^2 + a_2 = B_2, \quad \dots \quad (35)$$

$$-\frac{d^2a_1}{dx^2} + 2a_1^3 - 3a_1a_2 + a_3 = B_3; \quad \dots \quad (36)$$

while if by means of (27) and (2) we deduce

$$u_2 = a_1^2 - \frac{da_1}{dx}, \quad \dots \quad (37)$$

and then, by means of (37) and (27), eliminate  $u_2$  and  $u_1$  from (34), we obtain

$$\begin{aligned} -\frac{d^3a_1}{dx^3} + 6a_1^4 - 12a_1^2\left(a_1^2 - \frac{da_1}{dx}\right) + 3\left(a_1^2 - \frac{da_1}{dx}\right)^2 + 6a_2\left(a_1^2 - \frac{da_1}{dx}\right) \\ - 4a_1a_3 + a_4 = -\frac{d^3a_1}{dx^3} - 6a_1^4 + 3\left(a_1^2 + \frac{da_1}{dx}\right)^2 \\ - 6a_2\left(a_1^2 + \frac{da_1}{dx}\right) + 12a_1^2a_2 - 4a_1a_3 + a_4 = -\frac{d^3a_1}{dx^3} - 6a_1^4 \\ + 12a_1^2a_2 - 3a_2^2 - 4a_1a_3 + a_4 + 3\left(a_2 - a_1^2 - \frac{da_1}{dx}\right)^2 = B_4. \end{aligned} \quad (38)$$

Hence, comparing (35) with (24), (36) with (25), and (38) with (26) and (24), we see that

$$B_2 = \square, \quad \dots \quad (39)$$

$$B_3 = \triangle, \quad \dots \quad (40)$$

$$B_4 = \nabla + 3\square^2; \quad \dots \quad (41)$$

and it will be observed that, in virtue of (27), when the transformation is such as to cause the second term of the transformed quantoid to disappear, then  $B_2, B_3, \dots B_m$  are the successive coefficients of the transformed quantoid divided by their respective coefficients of binomial development, viz.

$$n \frac{n-1}{2}, \quad n \frac{n-1}{2} \frac{n-2}{3}, \quad \&c.$$

9. Thirdly, these results may be accounted for, and an indefinite series of similar results anticipated, *à priori*. To this end I recur to the critical functions of algebra, which may be regarded either as functions of the differences of the roots of an equation, or, independently of the existence of any root of any equation, as results of transformation. Regarded in the latter point of view, there is a mode of deriving a critical function from a quantic which suggests an analogous mode of deriving a criticoid from

a quantoid. For, let

$$f(x+a_1)=a_0(1, a_1, a_2, \dots a_n \text{X} x, 1)^n; \quad . \quad . \quad . \quad (42)$$

then we have

$$f(x)=a_0(1, a_1, a_2, \dots a_n \text{X} x-a_1, 1)^n, \quad . \quad . \quad . \quad (43)$$

or, which is the same thing,

$$f(x)=a_0(1, 0, B_2, B_3, \dots B_n \text{X} x, 1)^n, \quad . \quad . \quad . \quad (44)$$

provided only that

$$B_2=a_2-a_1^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

$$B_3=a_3-3a_1a_2+2a_1^3, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46)$$

$$\&c. = \&c.;$$

and  $B_2, B_3, \&c.$  ( $B_1$  vanishes identically) are the primary critical functions. That they are critical in this sense, viz. that they remain unaltered under the linear substitution, appears from the circumstance that

$$f(x+A_1)=a_0(1, A_1, A_2, \dots A_n \text{X} x, 1)^n \quad . \quad . \quad (47)$$

leads to

$$f(x)=a_0(1, 0, B_2, B_3, \dots B_n \text{X} x, 1)^n, \quad . \quad . \quad . \quad (48)$$

which is identical with (44). We have in fact a set of relations,

$$B_2=a_2-a_1^2=A_2-A_1^2, \quad . \quad . \quad . \quad . \quad . \quad (49)$$

$$\&c. = \&c. = \&c.,$$

which show that the transformation of  $f(x)$  by the linear substitution leaves the critical functions unaltered.

10. We may treat the quantoid

$$f(e^{fa_1dx}y)=e^{fa_1dx}(1, a_1, a_2, \dots a_n \text{X} \frac{d}{dx}, 1)^ny \quad . \quad . \quad (50)$$

in an analogous manner, remembering that a factorial now replaces the linear substitution. Thus we have

$$f(y)=e^{fa_1dx}(1, a_1, a_2, \dots a_n \text{X} \frac{d}{dx}, 1)^n\left(\frac{y}{e^{fa_1dx}}\right). \quad . \quad . \quad (51)$$

Now

$$\begin{aligned} \frac{d^n}{dx^n}(e^{-fa_1dx}y) &= e^{-fa_1dx} \left\{ \frac{d^ny}{dx^n} - na_1 \frac{d^{n-1}y}{dx^{n-1}} \right. \\ &\quad \left. + n \frac{n-1}{2} \left( a_1^2 - \frac{da_1}{dx} \right) \frac{d^{n-2}y}{dx^{n-2}} - \&c. \right\}, \quad . \quad . \quad (52) \end{aligned}$$

$$na_1 \frac{d^{n-1}}{dx^{n-1}} (e^{-f^{a_1 dx}} y) = e^{-f^{a_1 dx}} \left\{ na_1 \frac{d^{n-1}y}{dx^{n-1}} - n(n-1)a_1^2 \frac{d^{n-2}y}{dx^{n-2}} + \&c. \right\}, \quad (53)$$

$$n \frac{n-1}{2} a_2 \frac{d^{n-2}}{dx^{n-2}} (e^{-f^{a_1 dx}} y) = e^{-f^{a_1 dx}} \left\{ n \frac{n-1}{2} a_2 \frac{d^{n-2}y}{dx^{n-2}} - \&c. \right\}. \quad (54)$$

Hence, collecting results and making the proper substitutions and changes in (51), that equation becomes

$$f(y) = \frac{d^ny}{dx^n} + n \frac{n-1}{2} \left( a_2 - a_1^2 - \frac{da_1}{dx} \right) \frac{d^{n-2}y}{dx^{n-2}} - \&c., \quad (55)$$

which may be replaced by

$$f(y) = (1, 0, B_2, B_3, \dots B_n \frown \frac{d}{dx}, 1)^ny. \quad (56)$$

But if we transform the quantoid

$$f(e^{f^{A_1 dx}} y) = e^{f^{A_1 dx}} (1, A_1, A_2, \dots A_n \frown \frac{d}{dx}, 1)^ny. \quad (57)$$

in such manner as to deprive it of its second term, we shall be conducted to (56). Thus, as the transformation of the quantic by the linear substitution left the critical functions unaltered, so the transformation of the quantoid by the factorial substitution leaves the criticoids unaltered. In either case the primary transformation is such as to deprive the quantic or quantoid of its second term; and the critical functions and criticoids so obtained are what I term primary. I do not continue the development commenced in (55), because the criticoids of the second, third, and fourth degrees are hereinbefore exhibited.

11. Independently, then, of the resolution of a quantic into linear factors, or of a quantoid into linear symbolical factors, and whether such resolution be theoretically possible or not, there are certain functions of the coefficients possessing characteristic properties. Such functions are obtained by means of the primary transformation, and are invariable under the appropriate (linear or factorial as the case may be) substitution. They admit of a third definition, which may be expressed as follows.

12. If, after expanding the sinister of (13), we replace the indices of  $a$  by suffixes (i. e. write  $a_c$  instead of  $a^c$ ) and leave the indices of  $u$  standing (i. e.  $u^c$  remains  $u^c$ ), and if we call the sinister, so modified, the quantic corresponding to  $\Lambda_m$ , then a critical function is a function wherein the substitution for each coefficient  $a_r$  of its corresponding quantic  $\Lambda_r$  causes no change in the function, from which, indeed,  $u$  disappears. Again, if we divide either side of (8) by  $u$  and call the quotient on the sinister

the quantoid corresponding to  $A_m$ , then a criticoid is a function wherein the substitution for each coefficient  $a_r$  of its corresponding quantoid  $A_r$  causes no change in the function, from which  $u$  disappears. Thus defined, critical functions and criticoids are seen to be porismatic in their character.

“Oakwal” near Brisbane, Queensland,  
Australia, November 18, 1869.

XXVII. *On the Thermal Energy of Molecular Vortices.* By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS.L. & E., &c.\*

§ 1. **O**BJECT of this Paper.—In a paper “On the Mechanical Action of Heat,” which I sent to the Royal Society of Edinburgh in December 1849, and which was read in February 1850, it was shown that if sensible or thermometric heat consists in the motion of molecular vortices supposed to be arranged in a particular way, and combined in a particular way with oscillatory movements, the principles of thermodynamics, and various relations between heat and elasticity, are arrived at by applying the laws of dynamics to that hypothesis†. The object of the present paper is to show how the general equation of thermodynamics and other propositions are deduced from the hypothesis of molecular vortices, when freed from all special suppositions as to the figure and arrangement of the vortices and the properties of the matter that moves in them, and reduced to the following form:—*That thermometric heat consists in a motion of the particles of bodies in circulating streams with a velocity either constant or fluctuating periodically.* This, of course, implies that the forces acting amongst those particles are capable of transmitting that motion.

§ 2. *Steady and Periodical Component Motions.*—A vortex, in the most general sense of the word, is a stream or current which circulates within a limited space. Conceive a closed vessel of any figure and volume to be filled with vortices or circulating streams, the mean velocity of circulation in each such stream being the same; and let the velocities of the moving particles be either constant or periodic. How complex soever those motions may be, they may be resolved into the following component motions—a motion of steady circulation with the uniform velocity already mentioned as the mean velocity, and a motion consisting in periodical fluctuations of velocity. Those two component motions may be called respectively the *steady circulation* and the *disturbance*.

\* From the Transactions of the Royal Society of Edinburgh. vol. xxv. Communicated by the Author.

† Transactions of the Royal Society of Edinburgh, 1850, vol. xx.



§ 3. *Mean Pressure due to Centrifugal Force.*—Let an elementary circulating stream (that is, a circulating stream of indefinitely small sectional area) be supposed to flow round and round in an endless tube with the uniform velocity  $w$ ; let  $\rho$  denote the density of the stream,  $d\sigma$  the sectional area. Consider two cross sections of the stream at which the directions of motion of the particles are contrary; and consider what resultant forces are exerted by the stream on the two parts into which those two cross sections divide the tube. The mass of matter which flows through each cross section of the tube in a unit of time is

$$\rho w d\sigma;$$

and in each unit of time a mass of matter of that amount has its velocity reversed. The force required in order to produce that reversal of velocity is of the following amount in absolute units,

$$2\rho w^2 d\sigma;$$

and such is the amount of each of the pair of inward pressures which the tube exerts on the stream, and of each of the pair of equal and opposite outward pressures exerted by the stream on the tube, tending to pull it to pieces. It may be called the *centrifugal tension* of an elementary stream.

The velocity of the particles flowing in the stream may undergo periodical fluctuations, positive and negative alternately; these will cause periodical variations in the centrifugal tension; but the mean value of that tension will continue to be that given by the formula.

The *mean intensity of the centrifugal tension*, in a direction tangential to the stream, is found by dividing the amount given in the preceding expression by the collective area,  $2d\sigma$ , of the two cross sections, giving the following result,

$$\rho w^2.$$

Suppose now that the stream is cut by an oblique sectional plane, making the angle  $\theta$  with a transverse section. Then the area of that oblique section is greater than that of a transverse section in the ratio of  $1 : \cos \theta$ ; and the amount of the component tension in a direction normal to the oblique section is less than that of the total centrifugal tension in the ratio of  $\cos \theta : 1$ ; whence it follows that the mean intensity of the component centrifugal tension in a direction making an angle  $\theta$  with a tangent to the stream is

$$\rho w^2 \cos^2 \theta.$$

Next, suppose a vessel of any invariable volume and figure to be filled with vortices or circulating streams, the velocity of steady circulation being  $w$ , and the mean density  $\rho$ . The cen-

trifugal force will cause a pressure to be exerted in all directions against the inside of the vessel. To determine the mean intensity of that pressure irrespectively of periodical variations, conceive the contents of the vessel to be divided into two parts by an imaginary plane, and consider what will be the mean intensity of the force with which the circulating streams tend to drive asunder the portions of matter at the two sides of that plane. The plane will cut the streams that flow across it, some normally, others obliquely; and the tangents to those streams will have all possible directions relatively to a normal to the plane, subject to the condition, in the case of isotropic action, that the mean value of  $\cos^2 \theta$  must be the same for all positions of the plane. But the sum of the mean values of  $\cos^2 \theta$  for three planes at right angles to each other must be  $=1$ ; therefore the mean value of  $\cos^2 \theta$  is  $=\frac{1}{3}$ \*; and, finally, the *mean intensity of the centrifugal pressure* is given in absolute units per unit of area by the equation

$$p = \frac{\rho w^2}{3}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

§ 4. *Energy of Steady Circulation compared with Centrifugal Pressure.*—The actual energy† of the steady circulation in a unit of volume is expressed in absolute units of work as follows:—

$$\frac{\rho w^2}{2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which, being compared with equation (1), gives the following result:—

$$p = \frac{\rho w^2}{3} = \frac{2}{3} \cdot \frac{\rho w^2}{2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

that is to say, *the intensity of the centrifugal pressure on the unit of area is two-thirds of the energy of the steady circulation in a unit of volume.* This is one of the propositions of the paper of 1849–50, p. 151, eq. v.; but it is now shown to be true, not merely, as in the former paper, for molecular vortices arranged in a particular way, but for molecular vortices arranged in any way whatsoever, provided their action is isotropic and their mean velocity uniform.

A similar proposition has been proved, by Waterston, Clausius, Clerk Maxwell, and others, for the pressure produced by the

\* There is a well-known integration by which it is easily proved that, for a number of directions equally distributed round a point, the mean value of  $\cos^2 \theta$  is  $\frac{1}{3}$ .

† Called by Thomson and Tait the “Kinetic Energy.”

impulse of small particles flying about in all directions within a closed vessel and rebounding from its sides.

§ 5. *Vortices with Heterotropic Action.*—It is conceivable that, in solid bodies, molecular vortices may be so arranged as to produce centrifugal pressures of different intensities in different directions. In such cases it is to be recollected that the sum of the mean values of  $\cos^2 \theta$  for the obliquities of any set of lines to any three planes at right angles to each other is  $=1$ ; whence it follows that if  $p'$ ,  $p''$ , and  $p'''$  be the mean intensities of the centrifugal pressures in any three orthogonal directions, we have

$$p' + p'' + p''' = \rho w^2; \quad . \quad . \quad . \quad . \quad (4)$$

that is to say, *the sum of the mean intensities of the three centrifugal pressures in any three orthogonal directions is equal to twice the energy of the steady circulation in a unit of volume.* This proposition was not in the paper of 1849–50, which was confined to an isotropic arrangement of vortices.

§ 6. *Energy of the Periodical Disturbances.*—In the paper of 1849–50, p. 152, equation x., the energy of the periodical disturbances was taken into account by multiplying the energy of the steady circulation by a factor  $k$  greater than unity, thus giving for the total energy in a unit of volume the following expression,

$$\frac{\rho v^2}{2} = \frac{k \rho w^2}{2},$$

in which  $v^2$  denotes the mean of the squares of the resultant velocities of the particles with their combined motions. The values of the factor  $k$ , being the ratio which the total energy of the molecular motions bears to the energy of the steady circulation, are to be deduced in each case from the results of experiments on specific heat.

Thus the energy of the disturbances in a unit of volume is expressed by

$$(k-1) \frac{\rho w^2}{2} = \frac{3}{2} (k-1) p. \quad . \quad . \quad . \quad (5)$$

It may now be observed, in addition, that the energy of the disturbances may, and indeed must, be at times partly potential as well as actual—in other words, partly due to displacement as well as to fluctuation of velocity.

Let  $\pm u$  be the greatest fluctuation of velocity; then a particle of the mass unity has the energy  $\frac{u^2}{2}$  due to that fluctuation, in addition to the energy due to the steady circulation. It is only at the instants of greatest disturbance of velocity that the energy

is all actual; at every other instant the energy is partly potential. Hence  $v^2 = kw^2$  may be taken to denote, not the square of an actual velocity common to all the particles, but the value to which the square of the velocity of the particles would rise if all the energy of the disturbances, actual and potential, were expended in increasing the velocity of steady circulation.

§ 7. *Total Energy of Thermal Motions.*—The total energy of the motion compounded of steady circulation and periodical disturbances, in a unit of volume, is expressed, as in the paper of 1849-50, by the following equation, which also shows its relation to the centrifugal pressure,

$$\frac{k\rho w^2}{2} = \frac{3k}{2} p, \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

in which (to recapitulate the notation)  $\rho$  is the mean density,  $w$  the velocity of steady circulation; the centrifugal pressure  $p$  is expressed in absolute units of force on the unit of area; and the proportion  $k$ , in which the *total energy of thermal motions* exceeds the energy of steady circulation, is a quantity whose values and laws are left to be deduced from the results of experiment.

§ 8. *Determination of Centrifugal Pressures.*—The external pressure exerted by any substance, as we find it in nature, is a complex quantity, being compounded of the centrifugal pressure already mentioned, and of forces which may be classed together under the name of *cohesion*. To enable us to distinguish these components of the total pressure from each other, we have the principle that the centrifugal pressure varies as the density simply; whereas pressure or tension, or *stress* (to use a general term), arising from cohesive forces, must vary as some function of the density, of a higher order than the first power.

The *perfectly gaseous state* is an ideal state in which the substance exerts no external pressure except that which varies as the density simply—that is, centrifugal pressure. It is impossible to obtain a substance absolutely in the state of perfect gas; but the cohesive stress diminishes with increase of temperature and diminution of density in such a manner that it is possible, as is well known, to obtain substances approaching very nearly to the perfectly gaseous state, such as atmospheric air and various other gases; and the actual pressures of such nearly perfect gases may be used, either as approximate values of the pressures in the ideal state of perfect gas, or as data for calculating the latter kind of pressures by the method of limits. We thus have the means of determining, to a close approximation, the centrifugal pressure of a given substance at a given temperature and density; the



well-known formula being

$$\frac{p}{\rho} = \frac{p_0}{\rho_0} \cdot \frac{\tau}{\tau_0}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

in which  $\tau_0$  is the absolute temperature of melting ice,  $\tau$  the actual absolute temperature, and  $\frac{p_0}{\rho_0}$  the value of the quotient  $\frac{p}{\rho}$  at the temperature of melting ice, for the particular substance in question.

§ 9. *Temperature and Specific Heat.*—It is shown in the paper of 1849–50, that temperature, according to the hypothesis of molecular vortices, is a function of the quotient found by dividing the *energy of the steady circulation in a unit of mass* by a constant depending on the nature of the substance; which constant may be defined as the value which the energy of steady circulation in a unit of mass of the given substance assumes at a standard temperature, such as that of melting ice. The energy of the steady circulation in a unit of mass is

$$\frac{w^2}{2} = \frac{3}{2} \cdot \frac{p}{\rho};$$

whence it appears that the principle stated as to absolute temperature is expressed by equation (7), already given in § 8. The *total energy of the thermal motions* in a unit of mass is expressed by dividing equation (6) of § 7 by the density  $\rho$ ; hence that quantity of energy (denoted for shortness by  $Q$ ) is given in terms of the absolute temperature by the following equation,

$$Q = \frac{kw^2}{2} = \frac{3k}{2} \cdot \frac{p}{\rho} = \frac{3k}{2} \cdot \frac{p_0}{\rho_0} \cdot \frac{\tau}{\tau_0}, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The real specific heat of a substance, as defined in the previous paper, when expressed in units of work per degree, is

$$Jc = \frac{dQ}{d\tau} = \frac{3kp_0}{2\rho_0\tau_0} + \frac{3p_0\tau}{2\rho_0\tau_0} \cdot \frac{dk}{d\tau}, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

in which  $c$  is the real specific heat in terms of the minimum specific heat of liquid water, and  $J$  Joule's equivalent, or the dynamical value of the ordinary thermal unit.

There is one part of the specific heat which is necessarily constant for a given substance in all conditions; and that is the part which expresses the rate of increase with the temperature, of the energy of the steady circulation alone in a unit of mass, viz.

$$\frac{d}{d\tau} \left( \frac{Q}{k} \right) = \frac{w^2}{2g\tau} = \frac{3p}{2\rho\tau} = \frac{3p_0}{2\rho_0\tau_0}, \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The part of the specific heat which depends on periodical disturbances is expressed as follows:—

$$\frac{d}{d\tau} \left\{ \frac{(k-1)Q}{k} \right\} = \frac{3(k-1)p_0}{2\rho_0\tau_0} + \frac{3p_0\tau}{2\rho_0\tau_0} \cdot \frac{dk}{d\tau}. \quad (11)$$

It is only by experiment that it can be ascertained whether this part of the specific heat is constant or variable. Experiment has proved that it is constant for the perfectly gaseous state, and nearly, if not exactly, constant for other conditions, but that its values for the same substance in the solid, liquid, and gaseous conditions are often different\*.

The apparent specific heat contains other terms, depending on the expenditure of energy in performing external and internal work, according to principles of thermodynamics which are now well known.

§ 10. *Examples of the Proportion in which the Total Energy of the Thermal Motions exceeds the Energy of the Steady Circulation.*—In the perfectly gaseous state, the coefficient given in equation (9) is the specific heat at constant volume; and as that quantity is known to be constant at all temperatures, the second term of the right-hand side of the equation disappears, and it is reduced simply to the following,

$$Jc = \frac{3kp_0}{2\rho_0\tau_0}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The specific heat, in dynamical units per degree, of a perfect gas under constant pressure, is expressed as follows,

$$Jc' = Jc + \frac{p_0}{\rho_0\tau_0} = \frac{p_0}{\rho_0\tau_0} \left( \frac{3k}{2} + 1 \right); \quad . \quad . \quad . \quad (13)$$

and the ratio in which the latter coefficient is greater than the former is therefore

$$\frac{c'}{c} = 1 + \frac{2}{3k}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

whence we have the following formulæ for deducing the proportion  $k$ , borne by the total energy of the thermal motions to the energy of the steady circulation, from the ratio  $\frac{c'}{c}$  as determined

\* According to the nomenclature used by Clausius, the phrase “real specific heat” is applied to that part only of the specific heat which is necessarily constant for a given substance in all conditions. Hence, if that nomenclature were adapted to the hypothesis of molecular vortices, the term real specific heat would be applied to the coefficient given in equation (10) only, and that given in equation (11) would be considered as part of the apparent specific heat.

by experiment,

$$k = \frac{2}{3\left(\frac{c'}{c} - 1\right)}. \quad (15)$$

This method is applicable only to substances that are nearly in the perfectly gaseous state.

There is another method, applicable to the same class of substances, which is expressed as follows:—

$$k = \frac{2\rho_0\tau_0 Jc}{3p_0}. \quad (16)$$

This second method may be applied to liquids and solids also, under the following conditions: the quantity  $\frac{p_0}{\rho_0\tau_0}$  is to be calculated as for the perfectly gaseous state; and the specific heat  $c$  must be nearly constant.

The ratio which the energy of periodical disturbances in a unit of volume bears to the centrifugal pressure may be interesting in connexion with hypothetical views of the constitution of matter. It is expressed as follows:—

$$\frac{3(k-1)}{2}. \quad (17)$$

The following are some examples of the results of calculations by formulæ (15) and (17):—

Substance.	$\frac{c'}{c}$ .	$k$ .	$\frac{3}{2}(k-1)$ .
Atmospheric air . . . .	1·408	1·634	0·951
Nitrogen . . . . .	1·409	1·630	0·945
Oxygen . . . . .	1·400	1·667	1·000
Hydrogen . . . . .	1·413	1·614	0·921
Steam-gas . . . . .	1·297	2·242	1·863

§ 11. *General Equation of Thermodynamics.*—In the paper of 1849–50, pp. 158–164, the general equation of thermodynamics (equation 6 of that paper, p. 161) is deduced from the hypothesis of molecular vortices, on the supposition of a special form and arrangement of the vortices. In a subsequent paper, “On the Centrifugal Theory of Elasticity,” read to the Royal Society of Edinburgh in December 1851 (Transactions, vol. xx. pp. 433–436), the same general equation (being equation 25 of the latter paper, p. 436) is deduced from the hypothesis of molecular vortices, without any special supposition as to the form and arrangement of the vortices, but with certain assumptions as to the laws of the elasticity of the matter which moves in them. In

a paper read to the British Association in 1865, and published in the Philosophical Magazine for October of that year, a further generalization is effected; and it is shown that the general equation of thermodynamics follows from the supposition that sensible heat consists in any kind of steady molecular motion within limited spaces, without any assumption either as to the figures of vortices, or as to the special properties of the matter that moves in them. The object of this section of the present paper is to show how the same general equation is deducible from the hypothesis of molecular vortices as stated at the commencement of the paper—that is, freed from all special suppositions except that of a steady circulation, combined with periodical disturbances of speed, whose energy may bear any proportion, constant or variable, to that of the steady circulation.

The forces by which an elementary circulating stream, whether flowing with a steady or with a fluctuating speed, is kept in a given state of motion and of a definite figure and dimensions, are equivalent in their action to a tension exerted at each cross section of the stream of an amount which, at a given cross section and at a given instant, is expressed in absolute units of force by the product of the mass which flows along the stream in a second into the velocity of flow at that cross section and instant. The mean value of the tension is the product of the same mass into the mean velocity—that is, into the velocity of steady circulation. Hence the *mean centrifugal tension*, as this force may be called, is proportional to the square of the velocity of steady circulation, and therefore to the *absolute temperature*; and the work done by the forces to which the virtual tension is equivalent, during a change of the figure and dimensions of all the elementary circulating streams in a given body, may therefore be expressed by multiplying the absolute temperature by the change in the value of a function, to be afterwards determined, of the dimensions, figure, and temperature. If to that function be added a function which is the integral of the increment of the energy of steady circulation divided by the absolute temperature, the sum is what I have elsewhere called the *thermodynamic function*. Let it be denoted by  $\phi$ , and let  $dQ$  denote the quantity of energy which must be communicated to the body in order to produce the increment  $d\phi$  in the thermodynamic function at the mean absolute temperature  $\tau$ ; then we have

$$dQ = \tau d\phi; \quad . \quad . \quad . \quad . \quad . \quad (18)$$

and this, when the proper value has been assigned to the thermodynamic function, is the *general equation of thermodynamics*. The process of finding the value of the thermodynamic function is well known, but a summary of it will be given here for the sake of completeness:—



Let  $dx$ ,  $dy$ ,  $dz$ , &c. denote changes in the dimensions of unity of mass of the body, of the nature of strain, such as dilatations and distortions; and let  $X$ ,  $Y$ ,  $Z$ , &c. denote the forces, of the nature of elastic stress, which the body exerts in the respective directions of such changes, so that while the thermodynamic function undergoes the change  $d\phi$  the external work done by unity of mass of the body is

$$Xdx + Ydy + Zdz + \&c.$$

Then, by the principle of the conservation of energy, it is necessary that the following expression should be a complete differential,

$$\tau d\phi - Xdx - \&c.;$$

whence it follows that the thermodynamic function  $\phi$  is the integral of the following set of partial differential equations\*,

$$\frac{d\phi}{dx} = \frac{dX}{d\tau}; \quad \frac{d\phi}{dy} = \frac{dY}{d\tau}; \quad \frac{d\phi}{dz} = \frac{dZ}{d\tau}, \&c.;$$

that is to say, the thermodynamic function has the following value,

$$\phi = \psi(\tau) + \int \frac{dX}{d\tau} dx + \int \frac{dY}{d\tau} dy + \&c.,$$

in which all the integrals are taken at constant temperature.

For a perfect gas at constant volume we have  $dQ = Jc d\tau$ , in which  $Jc$  is the dynamical value of the specific heat of the gas at constant volume; and consequently  $\psi(\tau) = Jc \text{ hyp. log } \tau$ ; and the same is the value for any substance which, at the temperature  $\tau$ , is capable of approaching indefinitely near to the perfectly gaseous condition. There is some reason for believing that all substances may have that property\*; but to provide for the possibility, pointed out by Clausius (Poggendorff's *Annalen*, vol. xvi. p. 73), of the existence of substances which at certain temperatures are incapable of approaching indefinitely near to the perfectly gaseous condition, we may make (as that author does)

$$\psi(\tau) = Jc \text{ hyp. log } \tau - \chi(\tau),$$

where  $\chi(\tau)$  is a function of the temperature, which becomes  $=0$  at all temperatures at which an indefinitely close approximation to the perfectly gaseous state is possible—thus giving for the complete value of the thermodynamic function,

$$\phi = Jc \text{ hyp. log } \tau + \chi(\tau) + \int \frac{dX}{d\tau} dx + \int \frac{dY}{d\tau} dy + \&c. \quad (19)$$

That expression may be abbreviated as follows:—Let  $U$  be the

\* See Phil. Mag. for December 1865.

† See *ibid.*

potential energy of the elastic stress of unity of mass of the body at constant temperature; then

$$\phi = Jc \text{ hyp. log } \tau + \chi(\tau) + \frac{dU}{d\tau}; \quad . \quad . \quad . \quad (20)$$

and the corresponding form of the general equation of thermodynamics is as follows:—

$$dQ = \{Jc + \tau\chi'(\tau)\}d\tau + \tau d \cdot \frac{dU}{d\tau} \cdot . \quad . \quad . \quad . \quad . \quad (21)$$

§ 12. *Conclusion.*—In conclusion, then, it appears that the special suppositions, as to matters of detail, introduced into the hypothesis of molecular vortices in the paper of 1849–50 are not essential to the deduction from that hypothesis of the principles of thermodynamics, but that such matters of detail may be left open to be determined by future investigations.

XXVIII. *On Illumination and Polarization in Transparent Substances.* By L. SORET.

MY DEAR TYNDALL,

Geneva, Jan. 24, 1870.

I HAVE been somewhat long in replying to your kind letter of the 5th of December; numerous occupations have prevented me from communicating to you my idea of how, by the presence of particles diffused in a transparent medium, we may theoretically explain the various phenomena of illumination and of polarization which are observed when a pencil of light passes through a transparent substance. I am far from believing that my theory is not open to objection, and I should be at no loss to criticise it myself and to raise doubts in respect of it. You know, moreover, how many delicate points are involved in all that concerns the theory of reflection. I think, nevertheless, that my views on this subject will not be uninteresting to you.

Let us see, then, how we can account for the action exerted by these particles.

I. The first hypothesis would be to assume that the vibratory motion of the æther is communicated to those minute particles which occur in the path of the pencil of light pretty much as sound-vibrations in air are communicated to solids, such as strings or membranes. The corpuscles would thus become centres of disturbance, sending in all directions vibrations of the same kind as those which constitute the pencil of light; if, more especially, this pencil were polarized, the waves emitted by the particles would consist of rectilinear vibrations parallel to the incident vibrations. The explanation of the phenomenon would

be the same as that given by M. Lallemand, except in what concerns the origin of these waves.

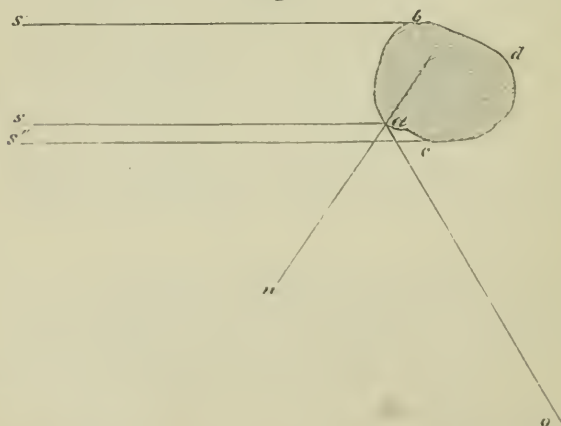
I confine myself here to mentioning this first hypothesis: as it has not hitherto been supported by any experimental proof, nor even by any analogy with other phenomena of light, it would be useless to discuss it at present.

II. A second hypothesis would assume that a phenomenon of diffraction is produced analogous to that of gratings. Let us imagine the transparent medium divided into layers perpendicular to the pencil of light; in each of these layers there are particles which should act like a very fine powder deposited on a transparent plate; that is to say, part of the light is no longer propagated in a right line, but diffuses itself in various directions. The vibrations producing this scattered light retain the form and the direction of the vibrations of the illuminating pencil; hence, if this is polarized, the scattered light will consist of rectilinear vibrations parallel to those of the pencil.

I am inclined to think that this diffraction really takes place, and contributes to the production of the phenomena observed, especially in directions which differ little from the direction of the illuminating pencil; but in any case the particles should also reflect light, and this effect ought to predominate over that which has been mentioned.

III. Let us, then, consider the reflected light. Let  $abcd$  (fig. 1) be a body illuminated by a pencil of parallel rays,  $s'b$ ,  $sa$ ,  $s''c$ .

Fig. 1.



Suppose the observer's eye placed in any position  $o$ , looking at the body  $abcd$ . For shortness' sake let us call *the angle of vision* the angle  $oas$  formed by the visual ray and the inci-

dent pencil. Let the *plane of vision* be the plane  $oas$  comprising the pencil of light and the visual ray.

If the dimensions of the body  $abcd$  are large as compared with the wave-lengths of the light, according to the ordinary laws of reflection the eye in the position  $o$  will only receive the rays reflected by a point  $a$ , such that the normal  $an$  to the surface of the body at this point is situated in the plane of vision, and that the angle of incidence  $san$  is equal to the angle of reflection  $nao$ . The reflected light will in general be partially polarized; and if the eye is in such a position that the visual angle is double the angle of polarization, the ray  $ao$  will be totally polarized, the plane of polarization coinciding with the plane of vision.

But we know that if the reflecting surfaces are not very great as compared with the wave-lengths of the light, the ordinary laws of reflection are no longer applicable, and each point of the reflecting surface may be considered as a centre of disturbance from which waves are really emitted—or, in other words, that the motion of the æther in all directions other than that of the reflected ray in the ordinary case of reflection is no longer annulled by interference. This is what results from the reasonings and experiments of Fresnel. If, then, the body  $abcd$  is very small and has dimensions comparable to the wave-lengths, the eye will receive more or less intense reflected rays proceeding from all points of the *illuminated surface* of the body. Thus, for instance, the visual angle being double the angle of polarization, the eye no longer receives merely the light totally polarized in the plane of vision reflected from the point  $a$ , but also non-polarized light, or light partially polarized, emitted by the other points of the surface illuminated by the incident pencil. There is no longer any reason why the maximum polarization should be exhibited when the angle of vision is twice the polarizing angle.

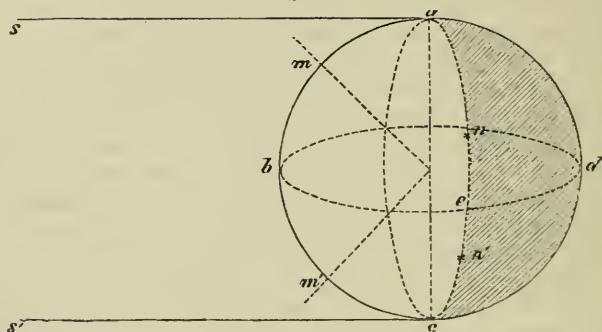
It is clear that this reasoning applies to all the corpuscles which are illuminated by the luminous pencil; thus a first source of difficulty is removed, that, namely, which you mentioned in your communication to the Royal Society on the 16th of December 1868.

IV. Let us now suppose that the pencil of light  $sa, s'c$  (fig. 2) is polarized in a horizontal plane perpendicular to the plane of the figure; it illuminates the very small particles, which first of all we will suppose to be spherical. Let us now examine what takes place in the case of one of these particles,  $abcd$ . The plane of polarization being horizontal, we will assume that the incident vibrations of the æther are rectilinear and vertical, and therefore parallel to the plane of the figure. The vibrations will be



reflected by each point of the illuminated part of the sphere ; and as the dimensions of this sphere are very small, each

Fig. 2.



point will become a centre of disturbance or of waves. The reflected vibrations forming the system of waves which proceed from one of the points of the spherical surface will be rectilinear, and their direction will be given by the laws of reflection of polarized light. (You see that I assume the *direction of the reflected rays* in these conditions to be the same as that which Fresnel assumes in the case of light reflected from large surfaces. The tenability of this hypothesis might in certain cases be contested ; but I think that even if it ought to be modified, that would make no change in the considerations I am about to develop, except for a part of those laid down in § VIII.)

Each reflected vibration may be regarded as resulting from three component vibrations—one vertical, the second horizontal and parallel to the luminous pencil, the third horizontal and perpendicular to the pencil.

Suppose the eye placed in the horizontal plane passing through the centre of the sphere. There is no reason why the vertical components of the various reflected waves should not reach the eye, which thus receives light polarized in a horizontal plane and proceeding in greater or less quantity from all the illuminated points of the reflecting corpuscle.

Let us pass to the horizontal components parallel to the pencil. Consider, for instance, the point *m* on the vertical great circle of the sphere parallel to the pencil, the normal at this point making an angle of  $45^\circ$  with the direction of the pencil. The incident vertical vibrations give rise at the point *a* to reflected waves entirely formed of vibrations horizontal and parallel to the pencil ; that is to say, the other components are null for this point. These vibrations are propagated in all directions, and therefore also in the direction of the eye placed in the horizontal plane

passing through the centre of the sphere. But, on the other hand, the point  $m'$ , which is symmetrical with the point  $m$ , also gives rise to waves of equal intensity formed of horizontal vibrations parallel to the pencil; only there will be a difference of phase of half a wave-length between the waves arising from the point  $m'$  and those from the point  $m$ . In fact the incident wave formed of vertical vibrations reaches the points  $m$  and  $m'$  in the same phase. Hence, if the molecules of æther in the incident vibration approach the reflecting surface at  $m$ , they are at the same time moving away from this surface at  $m'$ ; hence the motion will be from right to left in the vibration reflected by the point  $m$ , while it will be from left to right in the vibration reflected by the point  $m'$ . The reflected waves will therefore be in opposite phases, and will destroy each other in all points at equal distance from  $m$  and  $m'$ —that is to say, in all points of the horizontal plane passing through the centre of the sphere: thus the eye placed in this plane will not be affected by the horizontal vibrations proceeding from  $m$  and  $m'$ . In other corresponding points the effect will be the same (excepting that it will not be to the whole of the reflected vibrations, but merely to the horizontal component parallel to the beam that the reasoning applies). In fine, the horizontal components parallel to the luminous pencil proceeding from the illuminated part of the sphere situated above the plane of vision will be destroyed by interference with the same components proceeding from the part of the sphere which is below the plane of vision.

The same reasoning applies to the third components, horizontal and perpendicular to the pencil of light. Thus the effect produced by the point  $n$  will be destroyed by that of the corresponding point  $n'$  for the eye placed in the horizontal plane passing through the centre of the sphere.

In short, the eye in the position in question only receives light formed by the first components—that is, composed of vertical vibrations. In other words, the light is polarized in a horizontal plane.

It is easy to understand that, if the spherical corpuscles are small enough, this destruction by interference will be produced even outside the horizontal plane which passes through the centre of the sphere, provided the observer is not too far off. Thus when the plane of vision coincides with the plane of polarization of the pencil of light (which I will suppose to be very thin), all the illuminated corpuscles will transmit to the eye light consisting only of vertical vibrations. This is confirmed by experiment.

V. Always assuming that the corpuscles are spherical, let us examine what ensues when the horizontal pencil of light is

polarized in a vertical plane—that is to say, when the vibrations of the incident light are horizontal.

For the eye placed in the horizontal plane which passes through the centre of the sphere, the first vertical components will destroy each other by interference. Let us, in fact, take two symmetrical points,  $n$  and  $n'$ ; the incident horizontal vibrations will give rise to vertical reflected vibrations (or at least to vertical components); but, the inclination of the reflecting surface in reference to the horizontal vibrations being in opposite directions for the points  $n$  and  $n'$ , the reflected motion must also be in the opposite direction.

The second components, which are horizontal and parallel to the pencil, may reach the eye; there is no reason why they should interfere. The intensity of the light due to these components must be very feeble, as we shall subsequently see.

Nor will the third, horizontal components perpendicular to the pencil neutralize each other.

Thus the eye placed in the horizontal plane passing through the centre of the sphere will receive light from the second and third components. If the visual angle is  $90^\circ$ , the third components will produce no effect upon the eye, since they act longitudinally. Thus the eye in this position will only receive the impression of the horizontal components parallel to the pencil, which, as we have already said, are very feeble, and often quite imperceptible.

We readily deduce from this the explanation of the experiment which was suggested to you by Mr. Stokes\*.

If the visual angle is greater or less than  $90^\circ$ , the eye will receive the combined effect of the second and third components; but the direction of the resulting vibrations will be always parallel to the horizontal plane passing through the incident pencil. This result is also quite in accordance with experiment.

VI. If the illuminating pencil consists of natural light, we shall easily find what takes place by regarding it as composed of two luminous pencils polarized at right angles, one in the plane of vision (which I suppose to be always horizontal), the other in a perpendicular plane. We have the maximum polarization when the angle of vision is  $90^\circ$ ; for in this position the eye receives vertical components in large quantities, while from the horizontal components it only receives the impression of the second components of the reflected vibrations proceeding from the pencil polarized perpendicularly to the plane of vision.

\* In a private note Professor Stokes suggested to me the employment of polarized light to illuminate the actinic cloud, and I feel pleasure in giving him the credit of the suggestion. Had the suggestion not come to me, however, the result would have been substantially the same.—J. T.

Now, as we have already said and as we shall show, these components are very feeble.

If the angle of vision is less or more than  $90^\circ$ , then the horizontal components perpendicular to the pencil, proceeding from the pencil polarized perpendicularly to the plane, begin to act, and the polarization ceases to be at its maximum.

VII. Let us still suppose the plane of vision to be horizontal, and that the illuminating pencil is formed of natural light—that is to say, of two pencils polarized at right angles, one in the horizontal plane, the other in the vertical plane. Let us inquire why in the reflected light the components parallel to the pencil are very feeble as compared with the vertical components.

In the first place, even if the sphere had an absolute reflecting power, there would be a considerable predominance of the vertical components. In fact, on the vertical great circle  $aec$ , the pencil polarized in a horizontal plane will give rise to reflected vertical components at all points, except the two points at  $45^\circ$  from  $e$ . The pencil polarized in the vertical plane, on the contrary, will not produce at any part of the great circle components parallel to the pencil; the same is the case with the great circle  $abc$ . As regards the horizontal great circle  $be$ , the first pencil will everywhere produce only vertical components, while the second pencil will produce no components parallel to itself at  $b$ , nor will it at  $e$ ; it is only between these points that it will produce any.

But, further, this preponderance of the vertical components will be greatly increased by the fact that the reflecting power of the sphere is not perfect. Thus on the horizontal great circle  $be$  (which in the case of perfect reflecting power would produce most components parallel to the pencil), the intensity of the vertical components will be least at  $b$ , where its value will be proportional to  $\left(\frac{n-1}{n+1}\right)^2$ ,  $n$  being the refractive index of the corpuscles as compared with the transparent medium; from  $b$  this intensity will continually increase, up to  $e$ , where its value is proportional to 1. On the other hand, the intensity of the components parallel to the pencil will be zero at  $b$ , zero again at the point for which the angle of incidence is equal to the angle of polarization, zero again at the point  $e$ . Passing three times through zero, the intensity of these components must nowhere be considerable on the great circle  $be$ , &c.

The exact calculation of these intensities is very complicated. M. Cellérier has been kind enough to do it at my request for several values of the refractive index, and he has found that the vertical components greatly predominate over those parallel to the pencil.



VIII. I have hitherto always supposed that the corpuscles were spherical. Is this condition necessary for the explanation which I have given of the phenomena of illumination? I think not. For, on the one hand, whatever be the shape of the particles, the surface is always tarnished, and the phenomena of interference which I have pointed out will always be partially produced. On the other hand, we have not to consider a single particle. In a layer perpendicular to the luminous pencil there is a large number of particles; there must, then, in general be in this layer as many surfaces in a certain position as in the position symmetrical with it. These symmetrical surfaces will produce the interferences of certain components of the reflected vibrations, as if they belonged to the same particle.

This is the summary of my theory, or rather of my *attempt* at a theory.

Although this letter is already long, will you permit me to add a few words? You will have seen that M. Lallemand has answered (*Comptes Rendus*, Dec. 20) the note I communicated to the Academy (*Comptes Rendus*, Dec. 6), and that he still thinks the illumination of liquids and of solids should be attributed to a lateral propagation of the motion of the æther, and not to particles diffused in the transparent medium. The reasons which he adduces in support of his opinion do not seem to me conclusive; but I will not dwell upon this subject. I will merely mention that I have recently made a series of experiments by taking water and producing light precipitates in it by means of certain reagents. Whenever the precipitate was formed of particles so fine that they were not visible to the naked eye, and in such small quantity that the transparency of the water was not sensibly diminished in diffused light, I observed a great increase in the illuminating power of the line of light, with the same phenomena of polarization.

Thus I take water from the Lake of Geneva which has quite settled, and which has very little illuminating power; it contains some traces of chlorides in solution; I add to it, by means of a glass rod, a drop of an extremely dilute solution of nitrate of silver. The line of light becomes far more visible, and the polarization is complete. I add a larger quantity of nitrate of silver, and the water becomes a little dull; the line is still more distinctly visible, of a bluish tint; but if we examine with a Nicol's prism, in a suitable position, the extinction is no longer complete; the line is still seen, and is of a beautiful deep blue (almost the tint of the indigo of the spectrum).

It appears to me probable that this incomplete polarization is due, at least partially, to multiple reflections when the particles in suspension become very numerous. If water containing

chloride of silver is exposed to the sun's light, the precipitate changes its nature, the line is extremely distinct, but the phenomena of polarization are far less definite than before.

I do not see how these facts can be due to any other cause than that of particles in suspension.

Believe me,

My dear Tyndall,

Yours very sincerely,

L. SORET.

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XXIX. *On the Organic Dust of the Air.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I RECEIVED last night and translated rapidly this morning the following letter from M. De la Rive, which I trust will reach you in time for your forthcoming Number. I may add that the current Number of Fraser's Magazine contains a brief historic summary of this subject, to which I would refer the reader. I look forward with extreme interest to the publication of M. De la Rive's photometric observations.

JOHN TYNDALL.

Royal Institution,  
February 24, 1870.

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MY DEAR TYNDALL,

Geneva, February 22, 1870.

I have read with great interest the analysis of your lecture at the Royal Institution on the 21st of January, and admired the experiments by which you succeed in demonstrating that all the dust floating in the air of London is of an organic nature. Your conclusions as regards the probable cause of epidemic diseases remind me that I had announced the same consequences in an inquiry in which, by a process different from yours, I sought to prove the presence of organic dust in atmospheric air. Permit me to state in a few words the results of this inquiry.

I had followed with care the labours of Pasteur, who, like you, had deprived the air of its organic matter, sometimes by passing it through a red-hot tube, and sometimes by the employment of cotton wool. I had been struck by the fact that the air on high mountains, and more particularly on the summits covered with snow, does not contain any such matter. I was thus led to believe that the remarkable transparency of atmospheric air when it is charged with humidity, both before and after rain, arises from the fact that the organic particles which float constantly in the air, at least over plains, become transparent by absorbing

water, or fall to the earth on becoming heavier by the fact of that absorption. Finally, I found in the observations of De Saussure, and those of Humboldt, facts relative to the transparency of air which seemed to indicate that the clearness always exists in those places where organic matters are absent, or where the humidity annuls their influence.

These considerations led me to believe, as stated in the memoir communicated to the Academy of Sciences in Paris (*Comptes Rendus*, vol. lxiv. p. 1221, and *Ann. de Chim. et de Phys.* vol. xii. p. 243), that it would be interesting to embrace the transparency of the atmosphere in the number of meteorological elements which are subjected to regular determination, and thus to connect it in a precise manner with the other elements, such as the pressure, the temperature, the degree of humidity, the direction of the wind, and, above all, the hour of the day, the period of the year, or the seasons. This class of observations, I added, possessed interest, not only for science properly so called, but perhaps also for medicine, *as regards the hygiene of epidemic diseases*. It is very probable indeed, I said, that miasmas, the hydrogenous nature of which was demonstrated in 1834 by M. Boussingault, are due to these organic germs, whose presence would be thus established in a manner tolerably exact by the degree of transparency, more or less, of the air.

I expressed myself thus in 1867 ; but for the purpose of verifying my conclusions, I had previously sought to appreciate the variations of transparency by a photometer of a new construction, the description of which was given at the Meeting of the Société Helvétique in Geneva in 1865. (See my discourse on opening the Society, August 23, 1865, *Ann. de Chim. et de Phys.* vol. xxiv. p. 55.) This instrument, the detailed description of which is to be found in the memoir above referred to, has since served me in my regular observations. It confirms the idea which I entertained regarding the influence on the transparency of the air of organic matter, which floats in greater or less quantity according to the hour of the day, the period of the year, &c., and the effect of which is more or less abolished by the humidity of the air.

I intend to publish immediately the result of these observations, which continue to be regularly made at the Observatory of Geneva. Perhaps the Editors of the Philosophical Magazine would be good enough to make them known. They might be preceded by a description of the instrument which I name an *atmospheric photometer*, and by means of which the observations were made.

AUGUSTE DE LA RIVE.

XXX. *Proceedings of Learned Societies.*

## ROYAL SOCIETY.

[Continued from p. 153.]

June 17, 1869.—Lieut.-General Sabine, President, in the Chair.

**T**HE following communication was read :—

“On the Refraction-Equivalents of the Elements.” By J. H. Gladstone, Ph.D., F.R.S.

This paper is a continuation of the researches on refraction which have been already published by the author in conjunction with the Rev. T. Pelham Dale\*.

It is divided into two parts—the data, and the deductions. The data consist of the refraction-equivalents of some simple and many compound bodies, calculated from the indices observed by various chemists and physicists, or by the author himself, together with a series of observations on about 150 salts in solution. The method of examining these, and the nature of the inferences to be drawn from such experiments, have already been explained in the *Proceedings of the Royal Society*, 1863, pp. 440–444.

The deductions consist of a comparison of the evidence bearing on each elementary substance, beginning with carbon, hydrogen, and oxygen, which were in the first instance determined by Landolt. In the case of some elements all the means of calculation lead to the same number within probable errors of experiment; but in the case of others two or more different equivalents are indicated. Thus iron has one value in the ferrous and another in the ferric salts; and the more highly oxidized compounds of sulphur, phosphorus, arsenic, and nitrogen give different numbers from those given by their simpler combinations. The refraction-equivalent of potassium is estimated from a variety of sources, and the number thus arrived at is employed for the calculation of the other metals that give soluble salts, and for the radicals with which they are combined.

The following Table (p. 232) gives the general results of these deductions. The equivalents that have been deduced from only one compound, or of which the different determinations are not fairly accordant, are marked ? in the Table.

The specific refractive energy of a body is in some respects worthy of more consideration than the refraction-equivalent, since, being only the refractive index minus 1 divided by the density, it is a physical property independent of chemical theories as to the atomic weight. Among suggestive facts are noticed:—the extreme energy of hydrogen; the existence of pairs of analogous elements having the same, or nearly the same, energy—as bromine and iodine, arsenic and antimony, potassium and sodium, manganese and iron, nickel and cobalt; and that among the metals capable of forming soluble salts

\* *Phil. Trans.* 1863, p. 317.



there is some connexion between their power to saturate the affinities of other elements, and their power to retard the rays of light.

Element.	Atomic weight.	Refraction-equivalent.	Specific refractive energy.
Aluminium .....	27.4	8.4	0.307
Antimony .....	122	24.5 ?	0.201 ?
Arsenic .....	75	15.4 (other values?)	0.205
Barium .....	137	15.8	0.115
Boron .....	11	4.0	0.364
Bromine .....	80	15.3 In dissolved salts 16.9	0.191 or 0.211
Cadmium .....	112	13.6	0.121
Cæsium .....	133	13.7 ?	0.103 ?
Calcium .....	40	10.4	0.260
Carbon .....	12	5.0	0.417
Cerium .....	92	13.6 ?	0.148 ?
Chlorine .....	35.5	9.9 In dissolved salts 16.7	0.279 or 0.301
Chromium .....	52.2	15.9 In chromates 23 ?	0.305 or 0.441 ?
Cobalt .....	58.8	10.8	0.184
Copper .....	63.4	11.6	0.183
Didymium .....	96	12.8 ?	0.133 ?
Fluorine .....	19	1.4 ?	0.073 ?
Gold .....	197	24.0 ?	0.122 ?
Hydrogen .....	1	1.3 In hydracids 3.5	1.3 or 3.5
Iodine .....	127	24.5 In dissolved salts 27.2	0.193 or 0.214
Iron .....	56	12.0 In ferric salts 20.1	0.214 or 0.359
Lead .....	207	24.8	0.120
Lithium .....	7	3.8	0.543
Magnesium .....	24	7.0	0.292
Manganese .....	55	12.2 In permanganate 26.2 ?	0.222 or 0.476 ?
Mercury .....	200	20.2 ?	0.101 ?
Nickel .....	58.8	10.4	0.177
Nitrogen .....	14	4.1 In high oxides 5.3	0.293 or 0.379
Oxygen .....	16	2.9	0.181
Palladium .....	106.5	22.4 ?	0.210 ?
Phosphorus .....	31	18.3 (other values?)	0.590
Platinum .....	197.4	26.0	0.132
Potassium .....	39.1	8.1	0.207
Rhodium .....	104.4	24.2 ?	0.232 ?
Rubidium .....	85.4	14.0	0.164
Silicon .....	28	7.5 ? In silicates 6.8	0.268 ? or 0.243
Silver .....	108	15.7 ?	0.145 ?
Sodium .....	23	4.8	0.209
Strontium .....	87.5	13.6	0.155
Sulphur .....	32	16.0 (other values?)	0.500
Thallium .....	204	21.6 ?	0.106 ?
Tin .....	118	19.2 ?	0.163 ?
Titanium .....	50	25.5 ?	0.510 ?
Vanadium .....	51.2	25.3 ?	0.494 ?
Zinc .....	65.2	10.2	0.156
Zirconium .....	89.6	21.0 ?	0.234 ?

XXXI. *Intelligence and Miscellaneous Articles.*

ON COMETS AND METEORS. BY DANIEL KIRKWOOD LL.D.,  
PROFESSOR IN INDIANA UNIVERSITY\*.

THE comets which passed their perihelia in August 1862 and January 1866 will ever be memorable in the annals of science, as having led to the discovery of the intimate relationship between comets and meteors. These various bodies found revolving about the sun in very eccentric orbits may all be regarded as similar in their nature and origin, differing mainly in the accidents of magnitude and density. The recent researches, moreover, of Hoek, Leverrier, and Schiaparelli have led to the conclusion that such objects exist in great numbers in the interstellar spaces, that in consequence of the sun's progressive motion they are sometimes drawn towards the centre of our system, and that if undisturbed by any of the large planets they again pass off in parabolas or hyperbolas. When, however, as must sometimes be the case, they approach near Jupiter, Saturn, Uranus, or Neptune, their orbits may be transformed into ellipses. Such, doubtless, has been the origin of the periodicity of the August and November meteors, as well as of numerous comets.

In the present paper it is proposed to consider the probable consequences of the sun's motion through regions of space in which cosmical matter is widely diffused, to compare these theoretical deductions with the observed phenomena of comets, aërolites, and falling stars, and thus, if possible, explain a variety of facts in regard to those bodies which have hitherto received no satisfactory explanation.

1. As comets now moving in elliptic orbits owe their periodicity to the disturbing action of the major planets, and as this planetary influence is sometimes sufficient, especially in the case of Jupiter and Saturn, to change the *direction* of cometary motion, the great majority of periodic comets should move in the same direction with the planets. Now, of the comets known to be elliptical, 70 per cent. *have direct motion*. In this respect, therefore, theory and observation are in striking harmony.

2. When the relative positions of a comet and the disturbing planet are such as to give the transformed orbit of the former a small perihelion distance, the comet must return to the point at which it received its greatest perturbation—in other words, to the orbit of the planet. The aphelia of the comets of short period ought therefore to be found, for the most part, *in the vicinity of the orbits of the major planets*. The actual distances of these aphelia are as follows :—

\* Communicated by the Author, having been read before the American Philosophical Society, November 19, 1869.

TABLE I.—Comets whose Aphelion Distances are nearly equal to 5.20, the Radius of Jupiter's Orbit.

Comets.	Aphelion distances.	Comets.	Aphelion distances.
1. Encke's.....	4.09	7. 1766 II. ....	5.47
2. 1819 IV. ....	4.81	8. 1819 III. ....	5.55
3. De Vico's .....	5.02	9. Brorsen's .....	5.64
4. Pigott's (1783) ...	5.28	10. D'Arrest's .....	5.75
5. 1867 II. ....	5.29	11. Fay's .....	5.93
6. 1743 I. ....	5.32	12. Biela's .....	6.19

TABLE II.—Comets whose Aphelion Distances are nearly equal to 9.54, the Radius of Saturn's Orbit.

Comets.	Aphelion distances.
1. Peters's (1846 VI.) .....	9.45
2. Tuttle's (1858 I.) .....	10.42

TABLE III.—Comets whose Aphelion Distances are nearly equal to 19.18, the Radius of Uranus's Orbit.

Comets.	Aphelion distances.
1. 1867 I. ....	19.28
2. November meteors .....	19.65
3. 1866 I. ....	19.92

TABLE IV.—Comets whose Aphelion Distances are nearly equal to 30.04, the Radius of Neptune's Orbit.

Comets.	Aphelion distances.	Comets.	Aphelion distances.
1. Westphal's (1852 IV.)...	31.97	4. De Vico's (1846 IV.) ...	34.35
2. Pons's (1812) .....	33.41	5. Brorsen's (1847 V.).....	35.07
3. Olbers's (1815) .....	34.05	6. Halley's .....	35.37

The coincidences here pointed out (some of which have been noticed by others) appear, then, to be necessary consequences of the motion of the solar system through spaces occupied by meteoric nebulae. Hence the observed facts receive an obvious explanation.

In regard to comets of long period, we have only to remark that, for any thing we know to the contrary, there may be causes of perturbation far exterior to the orbit of Neptune.

3. From what we observe in regard to the *larger* bodies of the universe (a clustering tendency being everywhere apparent), it seems highly improbable that *cometic and meteoric* matter should be uniformly diffused through space. We should expect, on the contrary,

to find it collected in cosmical clouds similar to the visible *nebulæ*. Now this, in fact, is precisely what has been observed in regard both to comets and meteors. In 150 years, from 1600 to 1750, sixteen comets were visible to the naked eye\*, of which eight appeared in the twenty-five years, from 1664 to 1689. Again, during sixty years, from 1750 to 1810, only five comets were visible to the naked eye, while in the next fifty years there were double that number. The probable cause of such variations is sufficiently obvious. As the sun in his progressive motion approaches a cometary group, the latter must, by reason of his attraction, move toward the centre of our system, the nearer members with greater velocity than the more remote. Those of the same cluster would enter the solar domain at periods not very distant from each other—the forms of their orbits depending upon their original relative positions with reference to the sun's course, and also on planetary perturbation. It is evident also that the passage of the solar system through a region of space comparatively destitute of cometic clusters would be indicated by a corresponding paucity of comets. By the examination, moreover, of any complete Table of falling stars, we shall find a still more marked variation in the frequency of meteoric showers.

Previously to 1833, the periodicity of shooting-stars had not been suspected. Hence the showers seen up to that date were observed *accidentally*. Since the great display of that year, however, they have been regularly *looked for*, especially at the November and August epochs. Consequently the numbers recently observed cannot properly be compared with those of former periods. Now, according to the Catalogue of Quetelet, 244 meteoric showers were observed from the Christian era to 1833. These were distributed as follows:—

Centuries.	Number of showers.	Centuries.	Number of showers.
0 to 100	5	1000 to 1100	22
100 to 200	0	1100 to 1200	12
200 to 300	3	1200 to 1300	3
300 to 400	1	1300 to 1400	4
400 to 500	1	1400 to 1500	4
500 to 600	20	1500 to 1600	7
600 to 700	1	1600 to 1700	7
700 to 800	14	1700 to 1800	24
800 to 900	37	1800 to 1833	48
900 to 1000	31		

A remarkable secular variation in the number of showers is obvious from the foregoing Table. During the five centuries from 700 to 1200, 116 displays are recorded; while in the five succeeding, from 1200 to 1700, the number is only 25. It will also be observed that another period of abundance commenced with the eighteenth century. A catalogue of meteoric stonefalls indicates also a

\* See Humboldt's '*Cosmos*,' vol. iv. p. 538. The writer called attention to this variation as long since as 1861.



corresponding increase in the number of aerolites, which cannot be wholly accounted for by the increased number of observers. Now there are two obvious methods by which these variations may be explained. Either (1) the orbits of the meteoric rings which intersect the earth's path were so changed by perturbation towards the close of the twelfth century as to prevent the appulse of the meteoric groups with the earth's atmosphere, or (2) the nebulous matter is very unequally diffused through the sidereal spaces. That the former has not been the principal cause is rendered extremely probable by the fact that the number of epochs of periodical showers was no greater during the cycle of abundance than in that of paucity. We conclude, therefore, that during the interval from 700 to 1200 the solar system was passing through, or near, a meteoric cloud of very great extent, that from 1200 to 1700 it was traversing a region comparatively destitute of such matter, and that about the commencement of the eighteenth century it again entered a similar nebula of unknown extent.

The fact that the August meteors, which have been so often subsequently observed, were first noticed in 811 renders it probable that the cluster was introduced into the planetary system not long previously to the year 800. It may be also worthy of remark that the elements of the comet of A.D. 770 are not very different from those of the August meteors and the third comet of 1862\*.

Adopting Struve's estimate of the sun's orbital velocity, we find the diameter of the nebula traversed in 500 years to be fourteen times that of Neptune's orbit.

It is remarkable that, with the exception of Mars, the perihelia of the orbits of all the principal planets fall in the same semicircle of longitude—a fact which can hardly be regarded as accidental. Now, if the orbits were originally circular, the motion of the solar system through a nebulous mass not of uniform density would have the obvious effect of compelling the planets to deviate from their primitive orbits and move in ellipses of various eccentricities. It is easy to perceive, moreover, that the original perihelion points of all the orbits would be on that side of the system which had passed through the rarer portion of the nebulous mass. We have thus a possible cause of the eccentricity of the planetary orbits, as well as of the observed distribution of their perihelia†.

4. The particles of a cometic mass, being at unequal distances from the sun, will tend to move at different rates and in somewhat different orbits. This tendency will gradually overcome the feeble

\* The interval between the perihelion passage of 770 and that of 1862 is equal to nine periods of 121·36 years. Oppolzer's determination of the period of 1862 III. is 121·5 years. Hind remarks that the elements of the comet of 770 are "rather uncertain," but says "that the general character of the orbit is decided." It may be worthy of remark that a great meteoric shower, the exact date of which has not been preserved, occurred in 770.

† This suggestion is due to R. A. Proctor, F.R.A.S., the distinguished author of 'Saturn and its System.'

attractive force between the particles themselves. The most distant parts will thus become separated from the nucleus, and move in independent orbits. The motion of such meteoric matter will be in the same plane with that of the parent comet—the orbit of the former, however, being generally exterior to that of the latter. The connexion recently discovered between comets and meteors, and especially the fact that the period of the November group is somewhat greater than that of the comet of 1866, are in striking harmony with the views here presented.

5. Owing to this loss of matter, periodic comets must become less brilliant, other things being equal, at each successive return, a fact observed in regard to the comets of Halley and Biela.

6. The line of apsides of a large proportion of comets will be approximately coincident with the solar orbit. The point towards which the sun is moving is in longitude about  $260^\circ$ . The quadrants bisected by this point and that directly opposite extend from  $215^\circ$  to  $305^\circ$ , and from  $35^\circ$  to  $125^\circ$ . The number of cometary perihelia found in these quadrants up to July 1868 (periodic comets being counted but once) was 159, or 62 per cent., in the other two quadrants 98, or 38 per cent.

This tendency of the perihelia to crowd together in two opposite regions has been noticed by different writers.

7. Comets whose positions before entering our system were very remote from the solar orbit must have *overtaken* the sun in its progressive motion; hence their perihelia must fall for the most part in the vicinity of the point towards which the sun is moving; and they must in general have very small perihelion distances. Now, what are the observed facts in regard to the longitudes of the perihelia of the comets which have approached within the least distance of the sun's surface? But three have had a perihelion distance less than 0.01. *All* these, it will be seen by the following Table, have their perihelia in close proximity to the point referred to:—

TABLE V.—Comets whose Perihelion Distances are less than 0.01.

Perihelion passage.	Perihelion distance.	Longitude of perihelion.
d h		
1. 1668, Feb. 28 13.....	0.0017	$277^\circ \frac{1}{2}$
2. 1680, Dec. 17 23.....	0.0062	262 49
3. 1843, Feb. 27 9.....	0.0055	278 39

In Table VI. all but the last have their perihelia in the same quadrant.

TABLE VI.—Comets whose Perihelion Distances are greater than 0.01 and less than 0.05.

Perihelion passage.	Perihelion distance.	Longitude of perihelion.
d h		
1. 1689, Nov. 29 4 .....	0.0189	269° 41'
2. 1816, March 1 8 .....	0.0485	267 35
3. 1826, Nov. 18 9 .....	0.0268	315 31
4. 1847, March 30 6 .....	0.0425	276 2
5. 1865, Jan. 14 7 .....	0.0260	141 15

The perihelion of the first comet in Table VII. is remote from the direction of the sun's motion; that of the second is distant but 14°, and of the third 21°.

TABLE VII.—Comets whose Perihelion Distances are greater than 0.05 and less than 0.1.

Perihelion passage.	Perihelion distance.	Longitude of perihelion.
d h		
1. 1593, July 18 13 .....	0.0891	176° 19'
2. 1780, Sept. 30 22 .....	0.0963	246 35
3. 1821, March 21 12 .....	0.0918	239 29

With greater perihelion distances the tendency of the perihelia to crowd together around the point indicated is less distinctly marked.

8. Few comets of small perihelion distance should have their perihelia in the vicinity of longitude 80°, the point opposite to that towards which the sun is moving. Accordingly we find, by examining a Table of cometary elements, that with a perihelion distance less than 0.1 there is not a single perihelion between 35° and 125°, between 0.1 and 0.2 but 3, and between 0.2 and 0.3 only one.

Bloomington, Indiana, Sept. 14, 1869.

#### ON A POSSIBLE CAUSE OF THE GULF-STREAM.

BY ROBERT GILL, ESQ.

In every part of the equatorial oceans where the waters are free, a current is observed to set from east to west, or against the diurnal motion of the earth. The waters of the Atlantic in proximity to the equator are thus impelled with a certain velocity upon the eastern coast of South America; a portion of the equatorial current deviates towards the south, the remaining portion runs along the north-eastern coast of South America and enters into the Gulf of Mexico, whence it finally flows as the Gulf-stream. It appears, therefore, that the Gulf-stream may be considered as a derivation of the equatorial current, and that the latter may be a sufficient cause of the former. The direction in which the Gulf-stream flows (from south-west to north-east) is accounted for by the fact that, in advancing towards the north, it brings with it the velocity it possessed when further south, and consequently runs faster

than the zones of diminishing radius it successively meets; it thus describes a sort of spiral, through the composition of its own velocity with that of the parallels over which it flows. A similar deviation of the equatorial current towards the poles is also observed upon the eastern coasts of Africa and of Asia; so that in general we may say that in the rotation of the globe the waters of the equatorial regions tend to lag behind, and are in consequence deflected by the eastern coasts and impelled towards the poles. This flow of the waters towards the poles gives rise to counter-currents which restore them again to the equatorial zones.

If the superficial temperature of the globe were everywhere uniform, and if there were no friction between the waters and the earth, nor between portions of water moving with different velocities, this circulation, once begun, would last for ever, the globe would lose a certain quantity of mechanical work through the effort of the equatorial coasts, which would be communicated to the waters in the act of imparting to them the earth's velocity; but, on the other hand, the waters, being impelled upon the western coasts moving with a less velocity as their distance from the equator increases, would communicate to those coasts its acquired impetus, thus restoring the work expended upon them by the eastern coasts towards the equator. A continual interchange of force would thus be taking place between the solid globe and the waters on its surface; but in the supposed circumstances the compensation would be perfect, so that the general routine of the phenomena would remain constant.

But friction does in reality act in the phenomena, and consequently the compensation cannot be complete; the waters having lost through friction a part of the work which they contained, cannot restore entirely the force they absorbed in acquiring increased velocity; the equatorial current will therefore be retarded; consequently a greater expenditure of work will be required to impel it forwards, so that the motion of the whole system will gradually diminish, the force stored up in it being at length expended in friction. To maintain the motion of the currents and of the globe unimpaired, it is necessary that some exterior source of work be constantly in action capable of compensating that power which is changed through friction into heat or other forms of energy.

The temperature of the waters in the vicinity of the equator is always higher than that of the waters of the polar regions, for the reason that the sun's rays fall more copiously, surface for surface, towards the equator. The currents which flow towards the poles are therefore continually cooled, and those which return to the equator are continually heated; it follows that the waters flowing towards the poles gradually shrink in bulk, and that those which are returning gradually expand. As was stated above, if the temperature were uniform over the whole surface, there would be a perfect balance between the power abstracted by the currents in the vicinity of the equator and that restored by them in moving towards the poles (always supposing there were no friction); but the local differences of temperature give rise to notable effects. When the waters from the poles flow towards the equator, they not only slacken their



apparent velocity because they pass over regions moving more rapidly than themselves, but they undergo a further retardation and from a different cause: in expanding they are raised, they recede from the centre of the globe, and assume a greater radius; and this must evidently retard their angular velocity as the actual speed with which they moved is transferred from a less to a greater circumference. The expansion of the waters as they approach the equator thus causes an increase of rapidity in the equatorial current, which, as was said above, is caused by the slower relative speed of the water as compared with that of the earth. If the temperature of the waters flowing towards the poles remained constant, they would restore to the globe the stored-up energy acquired from the impulsion of the equatorial coasts; but as they approach the poles their temperature gradually falls and they shrink in bulk. This gives rise to a phenomenon contrary to that which happens in the currents flowing towards the equator: the water in shrinking approaches to the centre of the globe, it therefore transfers its actual velocity from a larger to a smaller circumference, and consequently its motion is increased relatively to that of the earth. Hence the waters flowing towards the poles tend to impart to the globe more mechanical work than they abstracted from it at the equator, and consequently to impel it forwards more than they retarded it. The heating of the equatorial regions thus tends to render the currents more rapid, and, as a consequence, to increase the earth's diurnal motion; and might be a means of compensating, in the manner described, the losses from friction &c., and of maintaining the motion of the waters and of the globe, which, as we have shown, would be gradually dissipated without a continual influx of mechanical work from without.

The source of such mechanical work, tending to maintain the motion of the waters and of the globe, is evidently the solar heat; but in what manner does it produce the dynamical effect?

The water is expanded by the solar heat; in the act of expanding it recedes from the earth's centre, that is to say, it is raised; and this operation requires an equivalent expenditure of work, as it implies the lifting of a weight. The solar heat not only heats the waters, but at the same time it imparts to them a certain quantity of potential energy in consequence of having lifted them. A portion of the solar heat is thus transformed into potential energy, which, as the waters cool and shrink in flowing towards the poles, takes the form of motion and communicates a greater velocity to the current, tending to compensate the loss of motion and of power which the currents and the globe are continually undergoing from the effects of friction.

According to this idea a portion of the solar heat received by the equatorial regions of the globe is converted into potential energy, in expanding and consequently raising the waters flowing from the poles towards the equator; and this potential energy, assuming the active form in the shrinking and consequent lowering of the waters as they flow towards the poles, tends to restore to the currents the motion lost through friction, and may be a means of supplying motion to the globe, or of maintaining its velocity of rotation.

Palermo, February 14, 1870.

Fig. 1.

Mont Tivoul

Aig. du Grand

Aig. du Petit



Fig. 3.

Fig. 4.

Fig. 2.



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XXXII. *On the "Veined Structure" of the Ice of Glaciers.*  
By HENRY MOSELEY, F.R.S., Canon of Bristol\*.

[With a Plate.]

THE veined structure of ice appears first to have been described by M. Guyot, in the year 1838. The following is his account of it, as he saw it on the glacier of Gries, which I translate from the work of M. Huber†:—

"I saw under my feet the entire surface of the glacier covered with furrows an inch or two wide cut in snowy ice, and separated from one another by ridges of harder and more transparent ice. It was evident that the mass of the glacier was here formed of these two different kinds of ice: the former (that of the furrows) was white and melted more rapidly; the other (that of the ridges) was more perfect, crystallized, transparent, and hard. The unequal resistance to melting of these two kinds of ice was the obvious cause of these depressions and elevations. After having followed them for several hundreds of metres I reached a crevasse 20 or 30 feet wide, which, cutting the furrows at right angles, exhibited down to the depth of 30 or 40 feet an admirable transverse section of this structure. As far as my eye could reach I saw the mass of the glacier composed of layers of the [opaque] white ice separated from one another by layers of the transparent ice, the whole forming a mass as regularly stratified as certain calcareous rocks." This is the veined structure of glaciers.

\* Communicated by the Author.

† *Les Glaciers*, p. 170. Paris, 1867.



The surfaces of the laminæ of transparent ice, of which the transverse sections are seen, as veins, on the sides of the crevasses, are of a curved form, rudely resembling the hollow of the bowl of a spoon\* cut across at its widest part, whose edge is to be supposed in contact with the surface of the glacier. If the lip be supposed to be more and more compressed in front, it will show the conoidal forms which the curved surfaces of transparent ice affect higher and higher up the glacier; and if it be sharpened and elongated, it will show those towards which these curved surfaces tend lower and lower down. Their forms have been well represented by the late Principal James Forbes in a woodcut at page 247 of his '*Occasional Papers on the Theory of Glaciers*,' which, by permission of his publishers†, I have copied (Plate III. fig. 6). The outcrop of these conoidal laminæ is most clearly seen in respect to those of them which are marked and distinguished from the rest by sand or dirt entangled in their edges, the softer white ice between which has been melted away. These are the (so-called) dirt-bands, first observed by the late Principal Forbes‡. They display (though in a manner requiring some attention to discover) the exact course of the veined structure where it intersects the surface of the glacier. Fig. 1 represents these dirt-bands as they appear on the *Glacier du Géant*. It is copied (by his permission) from one given by Professor Tyndall in his paper "*On the Veined Structure of Glaciers*," in the *Philosophical Transactions* for 1859 (part i. p. 305).

At the terminal face or end of a glacier the frontal inclination or inward dip of the spoon-like end of the surface of the blue transparent ice is very small—at first only perhaps  $5^{\circ}$ , but rising to  $10^{\circ}$ ,  $20^{\circ}$ , up to  $60^{\circ}$  or  $70^{\circ}$ , if we follow the medial line of the glacier, or axis parallel to its length. The veined structure is most energetically developed at the sides of the glacier; in the central portions it is comparatively feeble. "It is not to be regarded as a partial phenomenon, or as affecting the constitution of glaciers to a small extent only. Vast masses of some glaciers are thus affected: by far the greater part of the *Mer de Glace*, and its tributaries, is composed of this laminated ice"§. "I have satisfactorily made out," says Forbes||, "in every glacier which I have had the means of examining with that view, that the conoidal structure, however obscured, exists in all parts

\* *Occasional Papers on the Theory of Glaciers*, by Principal James Forbes, F.R.S., p. 247. Edinburgh: Black, 1859.

† Messrs. J. and A. Black. Edinburgh, 1859.

‡ Forbes's '*Occasional Papers*,' p. 246.

§ Tyndall, *Phil. Trans.* 1859, part i. p. 280.

|| *Occasional Papers*, p. 20.

of the true glacier, modified according to its length and breadth." Various explanations have been given of this characteristic veined structure of glaciers. It has been supposed that the strata formed horizontally by successive falls of snow and intervening thaws in the *névé* or half-frozen snow out of which glaciers form themselves, are by the unequal motions of the different depths of the glacier tilted up into vertical positions and then over, so as to incline forwards, and that the plane surfaces of these plates so tilted are moulded by the pressure of the ice into the hollow conoids of the veined structure. Mr. John Ball (than whom, in Alpine questions, there is no greater authority) advocates this theory, called that of stratification.

The late Principal James Forbes attributes the veined structure to the same cause as the ripple in a stream of water, and claims it in proof of the viscous theory of the descent of glaciers. He formed "a viscous fluid of plaster of Paris mixed with glue and ran it along an irregular channel or model valley which he had constructed for the purpose. The relative velocities of the top and bottom, and the sides and centre, of this pasty mass were shown by alternating layers of two coloured pastes. He observed the boundaries of the coloured pastes to be squeezed by their mutual pressures into greatly elongated curves whose convexities were in the direction of the motion; and in a medial section the retardation of the bottom and the mutual actions of the posterior and anterior parts shaped the bounding surface of the colours into a spoon-like form"\*.

Dr. Whewell seems to have thought that the retardation of the borders of a glacier would produce a tendency of the adjacent molecules to move towards its centre, and that the curved form of the veined surface is due to this tendency†.

Professor Tyndall has propounded a theory of the veined structure which, to distinguish it from the viscous theory of Forbes, he has named the "pressure theory." He has illustrated it as follows:—

"If a plastic substance, such as mud, flow down a sloping canal, the central portions will flow more quickly than the lateral ones, which are held back by friction. Now the flow may be so regulated that a circle stamped upon the central portion of such a mud-stream shall move downwards without sensible distortion, thus proving that the central mud is neither compressed nor stretched longitudinally; for if the former, the circle would be squeezed to an ellipse with its major axis transverse to the axis of the stream; and if the latter, it would be drawn out to an ellipse with its major axis parallel to the line of flow. A similar

\* Occasional Papers, p. 78.

† Tyndall, 'Glaciers of the Alps,' p. 396.

absence of longitudinal compression exists in many glaciers; and in such ice-streams there is no transverse central structure developed. But let a circle be stamped upon the mud-stream *near its side*; owing to the speedier flow of the centre, this circle must be distorted to an ellipse, because the part of the circle furthest from the side moves more quickly than the part nearest the side. Hence we shall have an ellipse formed with its major axis inclined downwards, indicating that the mud is compressed in one direction and expanded in another. An exactly similar state of things occurs in many glaciers: the ice near the sides is subjected to a pressure and tension like that here indicated; and we have marginal crevasses as the result of tension, while the veined structure is, at all events, associated with the pressure"\*.

In all these theories a connexion between the differential motion of the ice of glaciers and the veined structure is supposed; but in none of them is it recognized that the one is a necessary consequence and simple result of the other. It is the object of this paper to show that the differential motion is constantly producing in all parts of the ice those internal displacements which in certain parts the veins make visible—so that, under assumed conditions, the veined structure is a necessary result of the differential motion, and, conversely, the fact of the differential motion is proved by the existence of the veined structure—and that the force, whatever it may be, by which the first is produced is no other than that which, through it, is the cause also of the second. From this it follows that, as the differential motion is assumed to be wholly in the direction of the *length* of the glacier, no lateral motion of any kind, nor any lateral pressure, is necessary to that structural process, going on in all parts of the glacier, of which, in certain parts, the veined structure and the dirt-bands afford the evidence.

I have set off along the line A B, fig. 2, the measurements of Professor Tyndall, taken in links, across the Mer de Glace at "Les Ponts"†. The stations where he put stakes are numbered from 1 to 17. I have multiplied the mean daily motions of these stakes, observed in inches by Professor Tyndall, by 365; and reducing them to links, I have set them off from the same scale of equal parts along the dotted lines drawn from the several stations parallel to the length of the glacier; the points 1', 2', 3', 4', &c. show the terminations of these lines.

If stones were placed on the surface of the glacier at the points 1, 2, 3, 4, &c., and the ice retained the same mean daily motion as was observed by Professor Tyndall throughout the year, then

\* Phil. Trans. 1859, part i. p. 282.

† Ibid. p. 266.

at the expiration of it they would be found in the points 1', 2', 3', 4', &c. If, instead of being placed apart, the stones had been placed close together along the whole line, then 1', 2', 3', 4', 5', &c., instead of being a polygonal line, would have been an irregular curve. The irregularities in it may be supposed to be due to accidental causes, as, for instance, unevenness in the bottom of the channel along the line AB; so that, supposing these to be removed, or their effect as exhibited in AB to have been obliterated in the subsequent motion of the ice, the actual positions of the stones would have been those shown by the dotted line 1 1. That line would on this supposition represent the position of the ice at the end of a year, over which at the beginning of it the line AB was drawn. If, in like manner, 1' 1'', 2' 2'', 3' 3'', 4' 4'', &c. be set off equal to 1 1', 2 2', 3 3', 4 4', &c. respectively and in prolongation of them, then the dotted line 2 2 will represent the position of the ice which two years before was in AB; and if this process be repeated ten times, we shall finally arrive at the curve 10 10, representing at the end of ten years the position of the ice which at the beginning of those ten years was in AB; supposing the mean daily motion of the glacier to remain the same during that period. There is no account of the motion of the ice underneath the surface at Les Ponts, where these measurements of the motion of the surface-ice were made; but at a point higher up, near the Tacul, the eastern face of the side of the Glacier du Géant was exposed, and an ice precipice presented itself 140 feet high and nearly vertical. And on the 11th of August, 1857\*, Professor Tyndall and Mr. Hirst placed in it three stakes in the same vertical line—one at the top, the second at 35 feet, and the third at 4 feet from the bottom. The mean daily motions of these stakes respectively they found to be 6 inches, 4·59 inches, and 2·56 inches. I have multiplied these numbers by 365; and reducing the inches to links, and also the feet in the heights of the several stakes from the ground, I have set off in fig. 3, from the same scale as before, ED and EF equal to the heights of the first and second stakes, E1 equal to the mean annual motion of the lowest stake, F1' equal to that of the next higher, and D1 equal to that of the highest; and I have repeated the distances E1, F1', and D1 ten times along the lines E10, F10', and D10. The points 1 1'1, 2 2'2, 3 3'3, &c. show, therefore, the positions which the stakes would have occupied at the ends of ten successive years if the mean motion had remained the same; and if the stakes had been placed close together these polygonal lines would have become curved lines, representing the position taken up by the ice at the end of each successive year which started at first from AD. Now let it be imagined, as an illustration, that the ice at the side

\* Phil. Trans. 1859, part i. p. 270.



of the Mer de Glace at Les Ponts moves at the same rate as that of the side of the Géant does near the Tacul; E 10 10' 10 D, fig. 3, will then represent the side of the ice at A, fig. 2, or rather along the neighbouring line 1 1 2 3; D 10 being the top or surface-line, and E 10 the bottom one.

We can have no observations on the motion of the bottom ice elsewhere than at the sides of the glacier. Supposing, however, that it bears elsewhere the same proportion to the surface-motion as it does at the sides, it may be calculated that the mean annual motion of the bottom ice at stations 12 and 13, where the surface-ice moves fastest, is 457 links. In fig. 4 I have set off, on the same scale as before, the depth G H of the glacier in links, and from H the distances H 1, 1 2, 2 3, &c., each of 457 links, to represent the mean annual motions of the bottom ice in successive years at the stations 12 and 13, and, from G, G 1, 1 2, 2 3, &c. to represent the corresponding motions of the top ice, and from K corresponding distances to represent the motion of the ice at 35 feet from the bottom. The curved lines 1 1, 2 2, 3 3, &c. represent, therefore, on this figure, as the corresponding lines did on the preceding, the positions of the ice at the ends of ten successive years which at the beginning of them was in G H; so that if a boring had been made at that time at station 12 and filled with sand, then the sand would at the end of one year have been drawn out into the curve 1 1 (fig. 4), at the end of two years into the curve 2 2, &c., and at the end of ten years into the curve 10 10. And if a cut had been made across the glacier in the line A B and filled with sand, or with water which had frozen into a plate of transparent ice, then at the end of the first year that sand or that transparent ice would have arranged itself into a conoidal surface, of which 1 1 represents a horizontal section at the surface (fig. 2), and H 11 (fig. 4) represents a vertical section parallel to the axis of the glacier at station 12, and E 11 a section at the side A, and so of succeeding years; and at the end of ten years the plate of sand or of transparent ice, which at the beginning filled the vertical cut at A B, would have been drawn out and attenuated into the surface whose horizontal section at the top is 10 C 10 (fig. 2), its vertical section at the point of fastest motion 10 10 (fig. 4), and its section at the side 10 10 (fig. 3).

It will be observed of these surfaces, that they are of that spoon-shape which Principal Forbes has described as characteristic of the veined structure, that, as in that structure, their frontal dip continually diminishes as they descend the glacier, and that the surface indications of the structure are more energetically developed at the edges than in the centre. If we conceive a point in the channel of a glacier where such a cut is from

time to time made in it as it passes through that point, and if we conceive this cut to be constantly filled up with water which freezes to a plate of transparent blue ice inserted into the white opaque ice of the glacier, and, entering into the economy of its motion, is slowly drawn out and attenuated by the extension of the transverse plate into a conoidal surface of greater area from year to year, the extent of which attenuation may be estimated by a comparison of the lengths of the successive curved lines 1 1, 2 2, 3 3, 4 4, &c. in the diagram (fig. 2) with the straight line A B forming the base of the diagram, then we shall, I think, have conceived the formation of a structure identical with the veined structure. I do not think this filling of a cut with water the means by which a plate of blue ice is actually inserted transversely into the mass of the glacier. It may be effected by an extraordinary thrust to which the ice is somewhere subjected, by which the white opaque ice is melted across a transverse section, and then, when relieved of the thrust, frozen again into transparent ice, on Sir W. Thomson's principle; or we may conceive a transverse fissure to be filled with ice-débris or snow, and this ice-débris or snow so introduced into the substance of the white opaque ice, by dint of the tearing asunder, thinning out, extension, and kneading which it thus undergoes, to be converted into transparent ice\*, whilst the mass in which it is imbedded retains its character of white ice.

Professor Tyndall has shown that a process not unlike this takes place at an ice-fall. The ice pushed over the edge of the fall opens, as the leaves of a thick book open when it is placed on its back. These openings of the ice get filled with ice-débris, snow, and sometimes mingled dirt, as wool or any other compressible substance might be inserted between the leaves of the book. As the ice reaches the bottom of the fall, the force, whatever it is, which presses it forwards closes these openings, shuts up the book, and the inserted ice and snow, compressed into the substance of the glacier, submits to its law of the differential motion, and thins itself out and stretches and prolongs itself into the conoidal laminae of the veined structure.

Such an ice-fall is represented in fig. 5, which by the permission of Professor Tyndall I have copied from his paper "On the Veined Structure," in part i. of the Philosophical Transactions for 1859, p. 286.

Professor Tyndall thus describes it, as seen by him on the ice-fall of one of the branches of the lower glacier of Grindelwald:—

"The glacier, descending from its *névé*, reaches the summit of

\* Principal Forbes speaks (Phil. Mag. March 1859) of the "*glassy* structure of ice as attainable by the cohesion under pressure (especially if aided by motion with friction, or *kneading*) of the semiopaque and porous material of the glacier."

the fall, and is broken transversely as it crosses the brow. It descends the fall as a succession of broken cliffy ridges with transverse hollows between them. In these latter the ice-débris and dirt collect, partially choking up the fissures formed in the first instance. Carrying the eye downwards along the fall, we see, as we approach the base, these sharp ridges toned down, and a little below the base they dwindle into rounded protuberances which sweep in curves across the glacier. At the centre of the fall there is not a trace of the true [veined] structure to be observed. At the base of the fall it *begins* to appear—at first feebly, but soon becomes more pronounced, until finally, at a short distance below the fall, the eye can follow the structural grooving right across the surface of the glacier, while the mass underneath has become correspondingly laminated in the most beautiful manner”\*.

XXXIII. *On the Structure of the Human Ear, and on the Mode in which it administers to the Perception of Sound.* By R. MOON, M.A., Honorary Fellow of Queen's College, Cambridge.

[Continued from vol. xxxviii. p. 376.]

*On the Functions of the Membranous Labyrinth and of the Semicircular Canals.*

IN my previous papers I have proceeded on the assumption that, in those animals whose auditory apparatus includes a cochlea, the perception of ærially conveyed sounds is occasioned by the alternate ebb and flow of the *liquor Cotunnii* from the cochlear aperture, along the *scala*, to the vestibular aperture, and *vice versâ*; and my efforts have been mainly directed to pointing out the mode in which this alternate motion of the aural liquid is mechanically effected.

It is clear that this motion can in no degree be dependent upon the action, or even upon the existence, of the membranous labyrinth or of the semicircular canals. Whether it is capable of affecting the nerves expanded within the former is a different question, and one which will require consideration; but up to this point I have made no attempt towards explaining the functions, or accounting for the strange form exhibited by the membranous labyrinth. To this part of the subject I now propose to address myself.

Upwards of forty years have elapsed since the late M. Flourens demonstrated in the most conclusive manner, by means of experiments performed upon living subjects, that the semicircular canals in birds and in mammalia have functions totally unconnected with the sense of hearing.

\* Phil. Trans. 1859, part i. p. 284.

The researches to which I refer are contained in two memoirs read to the French Academy of Sciences in the year 1828, and inserted in the *Mémoires* of that body published in 1830; the first of which memoirs relates to the case of birds, the second to that of mammalia. Of the first, a Report\* was drawn up by the illustrious Cuvier, to whom, with MM. Portal and Duméril, the memoir was referred, and in whose presence the various experiments were repeated.

It results from M. Flourens's experiments, that in birds and in mammalia the nerves of the semicircular canals, whatever other functions they may possess, unquestionably exercise this, viz. of regulating the motions of the head, and of the animal generally, of enabling it to preserve its equilibrium and to move steadily in a given direction. No general description, however, can give an adequate idea of the remarkable phenomena developed in these memoirs.

I shall therefore in the first place describe in considerable detail the experiments and their results, which appear to have attracted very little attention from writers on aural anatomy†.

I shall next show how the effects described may be accounted for—a branch of the subject upon which M. Flourens has not entered.

I shall finally consider the question whether the functions thus shown to be possessed by the part of the animal economy under consideration are or not exercised concurrently with others of a totally different character—whether, in short, the semicircular canals are to any, and what, extent operative in producing perception of sound.

I. The semicircular canals in birds, as in mammalia, are three in number, two vertical and one horizontal, of which the largest is vertical and superior. “The smallest is situated horizontally. The *canalis minor*, or third of the series, is vertical. They contain corresponding tubes of vascular membrane; and they also possess enlarged ampullæ, on which the nerves are distributed in the same manner as in mammalia.” (*Cyclopædia of Anatomy and Physiology*, vol. i. p. 308.)

The superior vertical canal is described as directed from be-

\* *Ann. de Chim. et de Phys.* 1828. To a reference to this Report contained in the article on Sound in the *Encyclopædia Metropolitana* I am indebted for having my attention called to the subject of it.

† I find no mention of them in the admirable and very complete *résumé* of the subject drawn up by Mr. Wharton Jones for the ‘*Cyclopædia of Anatomy and Physiology*’ eight years after the publication of Cuvier’s Report; and I do not find the omission repaired in any later treatise on the subject which has come under my notice. Mr. Jones appears to have been acquainted with some earlier speculations of M. Flourens. See ‘*Cyclopædia*,’ vol. ii. p. 570.



hind forwards, while the inferior canal takes the opposite direction.

All the canals are enveloped in a thin bony coating (*tunique osseuse mince*).

When the horizontal canal on the left side of a pigeon was cut, a slight movement of the head from right to left and from left to right exhibited itself. This motion continued only for a short time, after which the animal recovered its usual demeanour (*allure*). Its senses, its intelligence, and the equilibrium of its movements remained perfect.

When the horizontal canal of the right side of the same bird was cut, the horizontal motion of the head suddenly reappeared, but with a rapidity and impetuosity such that the animal, entirely losing its equilibrium, fell, and for a long time rolled upon itself without being able to stand erect.

The violent motion of the head ceased after a time, and so long as the animal continued in repose its head continued in repose also; but the moment the animal moved, the motion of the head recommenced, and that motion always became stronger in proportion to the rapidity with which the animal attempted to move itself.

Thus, when standing still, the animal preserved its equilibrium; it lost it when it tried to walk; it lost it still more when it tried to walk quickly; it lost it entirely when it tried to fly. The animal feared to move: it saw well; it could still hear; it preserved all its instincts and all its intelligence; it ate and drank spontaneously, though with great pain.

M. Flourens observed this bird for a whole year. The wound in the head healed completely; the bird became very fat; but the motion of the head, the rolling upon itself, and the loss of equilibrium upon any sudden movement, however slight, always exhibited themselves.

When the inferior vertical canal on the left side of another pigeon was cut, there immediately ensued a slight but rapid movement of the head from below upwards and *vice versa*; but this only continued an instant. The animal, when left to itself, could hold itself erect: it walked and flew regularly, except that it experienced from time to time a sharp and sudden motion of the head backwards, which for the moment disturbed its equilibrium and went sometimes almost to the point of throwing it over on its back; at the end of some moments, however, this movement dissipated itself, or only reappeared after longer intervals.

When the inferior vertical canal on the right side of the same bird was cut, the vertical movement of the head reappeared with a violence and impetuosity in all respects similar to those

exhibited by the horizontal movement when the horizontal canal on both sides was cut. The movement up and down was almost continual: sometimes the head bent a little from one side to the other, as if to make a turn half round (*comme pour faire un demi-tour*); but the dominant direction of the movement was always from below upwards and *vice versá*.

When the pigeon was stationary, its equilibrium was maintained; and the better to preserve it, the animal rested its head on the ground; and it was always the top of the head bent back (*sommet de la tête renversée*) which was so supported.

The motion of the head always increased in activity along with the other motions of the body, and, on the other hand, disordered the latter to such an extent that all regular movement was impossible.

In this experiment, unlike what occurred in the former when the two horizontal canals were cut, the animal could neither walk nor fly. When thrown into the air, after some incoherent movements of its feet and wings, its whole frame stiffened, and it fell like an inert mass.

The animal never turned upon itself as when the horizontal canals were cut; but often, in its own despite, it fell over backwards upon its tail, and sometimes rolled for a long time in this direction.

This pigeon was kept for nearly a year. It ate and drank voluntarily (*de lui-même*), although it had infinite difficulty in governing its head for a moment so as to seize its food and drink. It could never fly; when it tried to walk at all fast, it fell and rolled on its back. Almost always it remained in the same place, the top of its head turned back resting on the ground or against the bars of its cage. The vertical movement of the head was always maintained, and always with almost equal intensity.

When the superior vertical canal on the left side of a third pigeon was cut, there immediately resulted a slight but rapid movement of the head from above downwards, and *vice versá*. This movement was of short duration, but soon recurred. Left to itself, the animal walked steadily or held itself erect; though liable from time to time to tumble forward.

When the corresponding canal on the right side of the same bird was cut, there instantly occurred a quick and violent movement of the head from above downwards and *vice versá*. This movement was accompanied, as in the former case, by disturbance of equilibrium, which, in like manner, ceased when the animal was in a state of repose, and recurred with its motion, increasing in rapidity when the latter was accelerated.

In all the experiments, when two corresponding canals were

cut, there resulted rolling of the eyeball and a convulsive motion of the eyelids.

When the third pigeon was operated upon, it did not roll sideways as in the case of the first, or tumble backwards as did the second, but tumbled forwards.

This bird was also preserved for a year.

A fourth pigeon had all the canals cut in both ears. There resulted immediately an impetuous (*fougueux*) and confused motion of the head in every direction—from above downwards, from below upwards, from right to left, and from left to right.

“Ce mouvement était d’une violence inouïe.” It so disturbed the equilibrium of the animal that it could only obtain some moments of repose by resting its head on the ground.

M. Flourens tried the effect of removing the osseous covering of the canals, leaving, I presume, in each case the lining membrane untouched, so as to prevent the lymph contained in the labyrinth from making its escape. None of the effects before described occurred in this case; but they exhibited themselves, though in a less degree, when the membranous labyrinth of the respective canals was *pricked*, the phenomena in each case being the same in character as when the corresponding part of the apparatus was cut through—thus showing that the phenomena above described were owing entirely to the action upon the [so-called] acoustic nerve expanded upon the membranous canals.

The same experiments were repeated upon other pigeons, upon pullets, sparrows, finches, yellow-hammers, linnets, tom-tits, &c. The results, though varying in intensity, were always identical in character.

After adverting to the circumstance of the experiments having been repeated in the presence of M. Duméril and himself, M. Cuvier gives his testimony concerning them in the following terms:—

“La section de chaque canal a produit effectivement les mouvemens bizarres qui sont annoncés dans le mémoire. Quelque surprenans, quelque inexplicables que sont ces faits, nous ne pouvons les révoquer en doute.”

The semicircular canals in mammals are enclosed in the petrous bone, the hardest and the most compact which their structure exhibits. In order that experiments similar to those practised upon birds might be extended to mammals, it was necessary in part to remove this bone; and on this account M. Flourens selected as the subject of his experiments young rabbits, animals which possess from birth the faculty of locomotion, at the same time that the bony envelope of their canals does not attain at that early stage of their career its full hardness.

For operating on the horizontal and posterior vertical canals,

rabbits of two months old were selected. In the case of the anterior vertical canal, which presented greater difficulty, the age had to be reduced to fifteen days.

In the first two cases the results of the experiments, with some unimportant variations, were the same in character as those of the corresponding experiments upon birds.

When one horizontal canal only was cut, the head of the animal in repose was always to the side of the section; but when both horizontal canals were cut, the head resumed its natural position. In the latter case, not merely the head, but the fore legs, and, indeed, the entire fore part of the animal, partook of the oscillatory movement from right to left and *vice versâ*.

It was found difficult to operate on the anterior vertical canal without injuring the brain, by which the results of the experiments were complicated with those flowing from the lesion of the latter. In the few cases in which this cause of error was avoided, the results of the experiments were precisely similar to those of the corresponding experiments on birds.

It is consolatory to think that the rabbits experimented upon did not seem to suffer the pain which was experienced by the pigeons; nevertheless M. Flourens does not appear to have succeeded in keeping alive the former beyond a month in one case, and a week in another.

II. Assuming that experiments operated on the semicircular canals of the human ear resembling in character those performed by M. Flourens upon pigeons and upon rabbits would be attended by like results, I now proceed to explain how the occurrence of such results may be accounted for.

The membranous labyrinth, I conceive, must be regarded in the light of a delicate balance, which, being suspended in the perilymph contained in the vestibule and semicircular canals, upon each movement of the head changes its position in the irregularly shaped bony cavity within which it is confined, and is thus enabled, in a manner presently to be described, to transmit to the mind or subordinate directing principle such intelligence respecting the motions of the head as may be necessary with a view to their regulation, variation, and maintenance.

The membranous labyrinth is supported by nervous filaments, which, leaving the inner wall of the osseous labyrinth, after traversing the perilymph, penetrate within as well the membranous vestibule (*sinus communis*) as the ampullæ, with one of which each canal is provided, and diffuse themselves over these parts of the membranous labyrinth to the exclusion of every other.

The experiments of M. Flourens show that motion of the head forwards is governed by the anterior vertical canals, mo-



tion backwards by the 'posterior vertical canals, while motions to the right and left are governed by the horizontal canals.

I apprehend that these effects are produced through the exclusive agency of the ampullæ, each of which (in form, as in function, resembling the bubble of a spirit-level) by its position indicates the motion of the head in the direction to which it has reference.

When the head moves, the membranous labyrinth will not implicitly follow its motion, but will lag somewhat behind, on account of the superior specific gravity of the endolymph as compared with the perilymph.

Thus, if the head move to the left, that end of the membranous ampulla of the left horizontal canal which is *nearest* to the vestibule will be forced into the corresponding portion of the surrounding osseous ampulla, the hard bony substance of which, pressing on the flexible integument of the membranous ampulla near its flask-like termination, will *constrict* the latter so as to cause the liquid contained within it to move in the direction of its length. Such a motion of the endolymph in the ampulla will necessarily excite the nerves expanded within it\*; and we can thus see how the change in the position of the head will be at once signalled to the brain.

The movement of the head (to the left) which we have been considering will produce in the right ear effects opposite to those which have just been described. In this case the end of the membranous ampulla of the right horizontal canal which is *furthest* from the vestibule will be forced into the corresponding portion of the surrounding bony ampulla. Consequently the end of the membranous ampulla which is furthest from the vestibule will in this case become constricted, and the flow of the endolymph will take place in a direction (with respect to the vestibule) opposite to that which it took in the former.

A second contemporaneous signal may thus be expected to be transmitted, which, although effected in a somewhat different manner, will have the same object as the signal from the left ear which we have just been considering, viz. to indicate that an exertion of the muscles by which the head is bent to the right will be necessary in order to restore the head to an erect position.

Such I conceive to be the mode of operation, and the special

\* "The filaments form a fine network on the outside of the ampullæ, and then piercing their parietes, are raised up within into a kind of crescentic screen, in order probably that they may be more exposed to the impulse of the vibrations descending along the aqueous endolymph of the semicircular tubes."—Penny Cyclopædia, 1837, vol. ix. p. 238. This description applies primarily to cartilaginous fishes, but is given as illustrative of what occurs in the case of the "superior animals, man included."

function, of the horizontal canals in the perfect organ when the head is moved to the left.

The application of the same principle of explanation to the same canals when the head moves to the right, and to the vertical canals when the head moves backwards or forwards, is so obvious that it need not now be dwelt upon.

When the head is held erect, or probably when recumbent in a horizontal position, I apprehend that each membranous ampulla will occupy the centre of its osseous envelope, so that at neither extremity of the former will pressure be exerted upon it by the latter.

If under these circumstances the horizontal membranous canal of the left ear were cut, I apprehend that the severed portions would *overlap* each other; so that the extremity of the membranous ampulla of the same canal which is *furthest* from the vestibule would be forcibly driven into the corresponding portion of the osseous ampulla. If this view be correct, it will be seen, by reference to the foregoing argument, that the same signal will have been conveyed to the brain as if the head had moved to the right; that is, warning will have been given to the muscles whose function it is to counteract a motion of the head to the right, to bring themselves into action; whence we might expect that a motion of the head to the left would ensue. As soon as this motion has been completed, however, it is to be anticipated that the sudden and perhaps violent motion of the poor animal under the pain of the operation would make this part of the membranous labyrinth oscillate in the contrary direction, so as to drive the membranous ampulla of the same canal into the opposite extremity of its osseous sac, and thus the signal to move the head to the right would be given.

In this way, I think, may be accounted-for the alternate motion of the head from right to left and from left to right which is stated to have resulted from cutting the left horizontal canal of the pigeon. We may see also how that further result of the experiment might be anticipated which is related to have occurred—that is, that the pigeon would by degrees learn to disregard the anomalous indications of its left ear, and rely wholly on those of the right ear alone, which preserved their normal character.

On the other hand, the cutting of both the horizontal canals would destroy all the means with which nature has provided the animal for ascertaining and measuring the direction and extent of the motions to right and left of its head, and would thus leave it in that state of incapacity to control those motions which is described by M. Flourens.

The effect of cutting the several vertical canals was, for the

most part, similar in kind to that of cutting the horizontal canals, though more violent in degree; and the explanation in all the different cases is in its main features the same.

When the ears are perfect, the effect of throwing the head back, upon the principles already explained, would be to force the extremity *nearest* to the vestibule of each posterior vertical ampulla into the corresponding portion of the surrounding osseous ampulla, and thus would be given a signal upon which, subject to the disposition of the controlling will, the muscles by which the head is moved *forwards* would be called into play.

On the other hand, when either of the last-named membranous canals is cut, on the principles above laid down, the effect would be to throw the end furthest from the vestibule of its ampulla into the corresponding portion of the osseous envelope; so that in the latter case a signal would be given of a character opposite to that given in the former; *i. e.* the tendency would be to make the animal fall backwards.

The mode in which, when both the posterior vertical canals were cut, the animal showed a disposition to turn its head upside down so as to rest the top of it on the ground, may be accounted for by supposing that in this way it placed in equilibrium the membranous ampullæ of the canals in question.

The mode of explanation in the case of the anterior vertical canals is obviously the same, *mutatis mutandis*, as in the case last considered.

If the views above set forth be correct, it is evident that we may look upon the membranous labyrinth in the light of a seventh sense\*, by which intelligence of the amount and direction of the motions of the head are communicated to the mind or subordinate governing principle. That there is a necessity for some such special provision as regards the motion of the head is sufficiently obvious when we reflect that we cannot judge of the position of the head with regard to surrounding objects in the way in which we can judge of that of the hand, the foot, or any part of our frame which can be contemplated by the eye. It might have been anticipated that the muscular effort necessary to move the head in a given direction would have afforded a sufficiently accurate measure of the effect produced by such effort. I think that M. Flourens's experiments prove conclusively that this is not so, and that a much more refined mode of measurement and adjustment has been provided for us—one, in fact, which takes cognizance of the effects actually produced, and not merely of the efforts made to produce them.

It will probably have been observed, however, that the mode of adjustment, so far as hitherto explained, is in one respect de-

\* The sense of muscular exertion being counted as one.

fective. To indicate fully motion in space, regard must be had to six directions, viz. right, left, backwards, forwards, upwards, downwards. Admitting that the membranous canals satisfy our requirements as to the first four directions, how are those for the last two provided for?

Upon this point I observe that the head cannot be moved up or down without a corresponding motion of the whole or of a large part of the body, for which a very considerable muscular effort is necessary in order to counteract or control the effect of gravity. It may very well be that such increased effort affords a satisfactory measure of the motion up and down, although a similar criterion as regards the horizontal motions would not be sufficiently exact.

I am inclined to think that it is at least one function of the nervous filaments expanded within the membranous sac and utricle to indicate the fact of motion generally, and that they may aid in measuring the vertical movements of the head—their operation in this respect being superseded, as regards the horizontal motions, by the more delicate apparatus afforded by the ampullæ. Any such action of the nerves of the sac and utricle must, of course, be enhanced by the cretaceous otolithes attached to them.

My remarks on this branch of the subject I shall conclude by pointing to the two following cases illustrative of it.

Every one who has had opportunity to observe the development of children in early infancy, must be aware that there is a time at which, although the child is able to maintain its head erect for a few moments, any attempt to change its position is followed by a wild movement of the head, partaking of the character of that described as taking place in M. Flourens's pigeons when all the canals had been cut.

The explanation of this circumstance I take to be, that although the child is able to keep its head erect, and has strength to move the head in different directions, yet it has not so mastered the indications afforded by means of the semicircular canals as to enable it to regulate the motion.

On the other hand, it is not uncommon to meet with persons of middle age, and, still more, with those advanced in life, who are subject to a nervous affection, under the influence of which, although ordinarily they can command the motions of the head like other persons, yet occasionally, and for a brief interval, they appear to lose control over them, the head moving involuntarily.

In explication of this peculiarity, I give from M. Flourens's second memoir his account of the demeanour of a rabbit, one of whose canals had been cut:—

“ Dans leur plus grande violence les oscillations de la tête sont  
*Phil. Mag.* S. 4. Vol. 39. No. 261. April 1870. S



très étendues : ces oscillations s'affaiblissent ensuite peu à peu ; un moment avant de cesser, *il n'y a plus qu'un léger tremblement qui représente tout à fait le tremblement de la tête qui s'observe dans certains vieillards.*"

Affections such as those last referred to I conceive to arise from paralysis (it may be only partial) of the semicircular canals of *one* ear.

III. I now propose to consider the question whether the functions above described exhaust the purposes of the membranous labyrinth—in other words, whether the membranous labyrinth actually forms part of the organ of hearing.

That, in man and those animals which possess a cochlea, all aërially conveyed sounds are perceived through the agency of the cochlea alone I think there cannot be the smallest doubt. The construction of the tympanal membrane, the bones of the ear, and the two foramina opening from the tympanal cavity into the labyrinth, abundantly prove that motion of the fluid in the cochlea from the round aperture towards the oval aperture, and *vice versâ*, is the special effect which aërially conveyed waves are calculated to produce ; and even if the nervous filaments of the membranous labyrinth hung loose in the perilymph which surrounds the latter, I cannot see that they would be in any way affected by such motion of the fluid in the cochlea. Still less can this be the case in the actual position of the extremities of the nervous filaments *within* the membranous labyrinth.

But it is not so clear that the nerves within the membranous labyrinth are not affected by undulations propagated through the bone of the skull—as, for instance, when a musical box is placed on the top of the head ; or rather it is not so clear whether these nerves do not, under the circumstances supposed, aid in the production of that sensation which we attribute to sound.

I am strongly inclined to take the negative view of the question last stated, and to regard the cochlea on the one hand, and the canals and membranous labyrinth on the other, as totally independent organs which, for constructive and, it may be, for other purposes, are locally connected—just in the same way as the ear and the nostril are connected, though the former is in no degree ancillary to the sense of smell\*. This, no doubt, is tantamount to the assumption that animals destitute of a cochlea are incapable of the perception of sound. It is to be observed, however, that it is not denied that when a sounding body is excited, animals in its neighbourhood which are possessed of sac and utricle only, or of those appendages and semicircular canals,

\* Mr. Whurton Jones testifies to a characteristic difference between the nerves of the cochlea and those of the membranous labyrinth. I do not desire, however, to urge too far this consideration.

receive a signal by which their conduct is regulated. All that is contended for is that such creatures are destitute of any thing corresponding to that affection of the mind which we designate as the hearing of sound.

Some writers, indeed, maintain that the sense of hearing is merely a refinement of the sense of touch. To this view of the subject I cannot subscribe. A table may vibrate under the influence of undulations which, when brought to bear upon the ear by means of a rod resting on the table, will produce the sensation of hearing. If, under the same circumstances, the table be touched with the finger, the nerves of the finger will be excited by the same vibrations; nevertheless we do not *hear* through the tips of our fingers.

As regards the effect produced by sounding bodies upon the nerves of the membranous labyrinth, whether in animals provided with a cochlea or those destitute of that adjunct—as, for instance, in fishes—I am quite disposed to admit that it may be merely a refined example of the sense of touch\*; but the perceptions excited in the human subject through the ear—and the same no doubt must in a degree apply to all animals endowed with a cochlea—appear as distinctly *sui generis* as are those derived through the eye, the nostril, or the palate.

6 New Square, Lincoln's Inn,  
January 15, 1870.

P.S.—The following account relating to the nervous apparatus of the ampullæ strikingly illustrates their adaptation to the performance of the delicate functions I have ascribed to them:—

“Dans les ampoules, Max Schultze a découvert, s'élevant hors de la surface interne de cet épithélium [that of the lining membrane of the ampullæ through which the nerves penetrate], des crins élastiques roides tout-à-fait particuliers. . . . Ils sont beaucoup plus longs que les crins ciliaires des cellules de mica, . . . fragiles, et se terminent en pointe très-fine. De petits crins de ce genre, fins et roides, sont évidemment et à un haut degré propres à suivre les mouvements du liquide, et, par suite, à produire une excitation mécanique dans les cordons nerveux implantés dans leurs bases, dans l'épithélium mou. . . .

“Les tumeurs épaisses, situées dans les vestibules où viennent se terminer les nerfs, présentent, d'après Max Schultze, le même épithélium dans lequel sont insérées les fibres nerveuses, mais point de crins, ou seulement de très-courts”†.

\* The fact of the antennæ of certain classes of Articulata, through which they are endowed with an exquisite sense of touch, being connected with what have hitherto been regarded as the organs of hearing, is strongly confirmatory of this view.

† *Théorie Physiologique de Musique, &c.*, par H. Helmholtz. Traduit par M. G. Quercourt. Paris, 1868: p. 172.

From the same work I take this further extract :—" La théorie des sensations auditives qui précède a été confirmée d'une manière intéressante par les observations et les expériences de V. Hensen sur les organes de l'ouïe chez les Crustacés. Ces animaux ont de petits sacs moitié fermés, moitié ouverts à l'extérieur, où les otolithes nagent librement dans une humeur aqueuse, et qui sont surmontés de petits crins rigides particuliers, reliés par leurs extrémités aux otolithes, formant une série ordonnée par ordre de grandeur. . . . On trouve aussi, chez beaucoup de crabes, de petits crins tout-à-fait analogues sur les parties libres du corps, et qui doivent être considérés comme les organes de l'ouïe. Ce qui le prouvent pour les crins extérieurs c'est en partie l'analogie de leur construction avec les crins des otolithes, en partie la circonstance qu'ils se relient aux nerfs de la même manière, et partent de la même souche que dans les otolithes. Enfin Hensen a trouvé que l'ouïe persistait après l'extirpation des sacs des otolithes dans le *Mysis* [the Opossum Shrimp], l'animal ne conservant que les crins extérieurs des antennes"\*.

Without questioning the action of sonorous undulations upon the organs referred to, I would suggest a doubt as to whether the sensations so produced can in any correct sense of the term be designated as sensations of hearing.

Recurring to the subject of the human ear, I would remark that the aërial waves emanating from a sounding body must necessarily produce an oscillation to and fro of the head in a vertical plane passing through the source of sound. Upon the principles I have above endeavoured to unfold, it is conceivable that this oscillation, however minute, may produce such an effect on the nerves of the ampullæ as in some degree to indicate the direction from which the sound proceeds—a view which many have entertained—although these nerves, as I conceive, do not in any way contribute to the sensation of hearing.

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XXXIV. *A new discussion of the Mathematical Theory of Oceanic Tides.* By the Rev. Professor CHALLIS, M.A., F.R.S., F.R.A.S.†

THE theory of ocean-tides which I gave in the Philosophical Magazine for last January conducted to formulæ which, under the assumed limitations, appeared to represent correctly some of the laws of tidal phenomena. But on proceeding to calculate the theoretical amount of tide, I found it to be so very small, that I came to the conclusion that the theory was contradicted by observation and must be abandoned. Also,

\* *Théorie Physiologique de Musique*, &c., par H. Helmholtz, p. 187.

† Communicated by the Author.

having reconsidered the argument from which it was inferred that the forms of the arbitrary functions contained in the expression for  $\phi$  might be determined by satisfying the given conditions of the problem *before* obtaining by integration the general value of that function, I became convinced that the intervention of the general value is indispensable, and that the forms of the functions must be ascertained by means of the equation

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0. \quad . \quad . \quad . \quad . \quad (\alpha)$$

The mode of using the equation for this purpose will be exemplified by the new solution of the problem I propose to give in this communication.

Although for the latter reason the theory of tides contained in the January Number must be pronounced to be erroneous, the general considerations by which it was prefaced may still be regarded as applicable to this and other hydrodynamical problems; and I may say also that this attempt was indirectly the means of suggesting the solution I am about to offer, which will, I hope, be found to be free from objection.

It will be supposed, as before, that the solid part of the earth is spherical, that it is wholly covered by water, the depth of which is uniform and small compared with the earth's radius, and that the attracting body revolves about the earth in the plane of the equator at its mean distance and with its mean angular velocity. In order to abstract from the earth's rotation, an equal angular velocity will be supposed to be impressed on the earth and the body in the opposite direction, so that the earth will have no motion. Centrifugal force will not be taken into account, because the waters under the action of this force alone would move as if they were solid, and consequently its effect, according to the preliminary considerations above referred to, cannot be included in an investigation which assumes that  $udx + vdy + wdz$  is an exact differential. Also the only *periodic* effect of centrifugal force is that which arises from the periodic motion of the water in latitude, and, under the circumstances supposed, is so inconsiderable in amount that, to avoid complexity, it will be disregarded. For the same reason the small effect of the spheroidal form of the earth is left out of account. It is evident, since the motion consists of small oscillations proper to a fluid, that we may put  $(d\phi)$  for  $udx + vdy + wdz$ .

This being understood, let the earth's centre be the origin of rectangular coordinates, and,  $\lambda$  being the north latitude, and  $\theta$  the longitude west from Greenwich, of any particle distant by  $r$  from the origin, let

$$x = r \cos \lambda \cos \theta, \quad y = r \cos \lambda \sin \theta, \quad z = r \sin \lambda.$$



If  $G$  be the usual measure of gravity at the upper surface of the fluid at the distance  $a$  from the centre of the spherical mass, the total attraction of the solid and fluid portions towards the centre at any point within the fluid at the distance  $r$  will be very nearly  $G\left(1 + k \frac{a-r}{a}\right)$ ,  $k$  being a certain constant depending on the ratio of the density of the water to the earth's mean density. If this ratio be one-sixth, which is known to be about its actual value, the calculation of the attraction shows that  $k=1$  nearly.

Let  $m$  be the attraction of the disturbing body at the unit of distance, referred to the same unit of measure as  $G$ ,  $R$  its mean distance,  $\mu t$  its angular distance westward from the meridian of Greenwich at the time  $t$  reckoned from Greenwich meridian transit,  $\mu$  being the excess of the earth's rate of rotation above the body's mean motion. Then, omitting powers of the ratio of  $r$  to  $R$  above the first, the usual investigation gives

$$X = -G\left(1 + k \frac{a-r}{a}\right) \frac{x}{r} + \frac{m}{R^3} \left(x(3 \cos^2 \mu t - 1) + \frac{3y}{2} \sin 2\mu t\right),$$

$$Y = -G\left(1 + k \frac{a-r}{a}\right) \frac{y}{r} + \frac{m}{R^3} \left(y(3 \sin^2 \mu t - 1) + \frac{3x}{2} \sin 2\mu t\right),$$

$$Z = -G\left(1 + k \frac{a-r}{a}\right) \frac{z}{r} - \frac{mz}{R^3}.$$

Hence in this case  $Xdx + Ydy + Zdz$  is an exact differential. Substituting  $(dF)$  for it, we have, by integration,

$$F = -G\left(r - k \frac{(a-r)^2}{2a}\right) + \frac{m}{2R^3} (x^2(3 \cos^2 \mu t - 1) + y^2(3 \sin^2 \mu t - 1) + 3xy \sin 2\mu t - z^2).$$

Since  $(dF) - (dp)$  is equal to  $\left(\frac{du}{dt}\right)dx + \left(\frac{dv}{dt}\right)dy + \left(\frac{dw}{dt}\right)dz$ , and this quantity is consequently an exact differential, if we represent it by  $(dQ)$ , we shall get by integration  $p = F - Q + \psi(t)$ . Hence, substituting for  $x, y, z$  in  $F$  their values in polar coordinates, it will be found that

$$\left. \begin{aligned} p = & -G\left(r - k \frac{(a-r)^2}{2a}\right) \\ & + \frac{mr^2}{2R^3} (3 \cos^2 \lambda \cos^2 (\theta - \mu t) - 1) - Q + \psi(t). \end{aligned} \right\} \quad (\beta)$$

The investigation in the January Number led to an expression for  $\phi$ , which, as already stated, did not satisfy the equation  $(\alpha)$ ; and although it gave results in accordance with observed laws

of tidal phenomena, it seemed to give too small an amount of tide. I was therefore induced to try whether, on substituting in the equation ( $\alpha$ ) an expression for  $\phi$  of the same form as that resulting from the previous investigation, viz.

$$F(r) \cos^2 \lambda \sin 2(\theta - \mu t),$$

the value of the function  $F(r)$  was determinable by the condition of satisfying that equation; and I found, in fact, that this condition is fulfilled if  $F(r)$  be deduced from the equation

$$\frac{d^2 \cdot F(r)}{dr^2} + 2 \frac{d \cdot F(r)}{r dr} - \frac{6F(r)}{r^2} = 0.$$

The integral of this equation is

$$F(r) = Cr^2 + \frac{C'}{r^3},$$

in which  $C$  and  $C'$  may be taken to be arbitrary constants. Consequently we have at once the following results:—

$$\phi = \left( Cr^2 + \frac{C'}{r^3} \right) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$\frac{d\phi}{dt} = -2\mu \left( Cr^2 + \frac{C'}{r^3} \right) \cos^2 \lambda \cos 2(\theta - \mu t),$$

$$u = \frac{d\phi}{dr} = \left( 2Cr - \frac{3C'}{r^4} \right) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$v = \frac{1}{r \cos \lambda} \frac{d\phi}{d\theta} = 2 \left( Cr + \frac{C'}{r^4} \right) \cos \lambda \cos 2(\theta - \mu t),$$

$$w = \frac{1}{r} \frac{d\phi}{d\lambda} = - \left( Cr + \frac{C'}{r^4} \right) \sin 2\lambda \sin 2(\theta - \mu t).$$

The arbitrary quantities  $C$  and  $C'$  must be capable of satisfying the given conditions of the problem; otherwise the solution fails. It will therefore be proper, in the next place, to try the solution by this criterion.

One condition to be satisfied is, that at the inferior boundary of the ocean the velocity  $u$  is zero at all points and at all times. Let  $b$  be the radius of this surface. Then evidently this will be the case if

$$2Cb - \frac{3C'}{b^4} = 0, \text{ or } \frac{C'}{C} = \frac{2b^5}{3}.$$

To fulfil another condition, recourse must be had to the expression already obtained for the pressure  $p$ .

In the subsequent reasoning, terms involving a higher power

of the disturbing force than the first, and the small term involving  $k$ , will be omitted. In that case  $Q = \frac{d\phi}{dt}$ , and the equation ( $\beta$ ) becomes

$$p = -Gr + \frac{mr^2}{2R^3} (3 \cos^2 \lambda \cos^2 (\theta - \mu t) - 1) - \frac{d\phi}{dt} + \psi(t). \quad (\gamma)$$

Now the expression for  $\frac{d\phi}{dr}$  shows that the vertical velocity is always zero where  $\lambda = \pm \frac{\pi}{2}$ , and consequently that at the two poles the radius  $r$  of the surface is constant. Let  $a$  be this value of the radius, and  $G$  be the attraction of gravity at these positions. Hence, since  $\frac{d\phi}{dt}$  also vanishes where  $\lambda = \pm \frac{\pi}{2}$ , if  $\varpi$  be the atmospheric pressure, which is supposed to be uniform and constant, we shall have

$$\varpi = -Ga - \frac{ma^2}{2R^3} + \psi(t).$$

This equation gives the value of  $\psi(t)$ , by substituting which in the equation ( $\gamma$ ), and putting for  $\frac{d\phi}{dt}$  its value

$$-2\mu C \left( r^2 + \frac{2b^5}{3} \right) \cos^2 \lambda \cos 2(\theta - \mu t),$$

the following result will be obtained:

$$\left. \begin{aligned} p - \varpi = & G(a - r) + \frac{m}{2R^3} \left( a^2 - r^2 + \frac{3r^2}{2} \cos^2 \lambda \right) \\ & + \left( \frac{3mr^2}{4R^3} + 2\mu C \left( r^2 + \frac{2b^5}{3r^3} \right) \right) \cos^2 \lambda \cos 2(\theta - \mu t). \end{aligned} \right\}. \quad (\delta)$$

The equation of the exterior surface of the ocean results from this equation by putting  $\varpi$  for  $p$ . It will thus be found, omitting terms containing  $m^2$  &c., that

$$\left. \begin{aligned} r = & a + \frac{3ma^2}{4R^3G} \cos^2 \lambda \\ & + \left( \frac{3ma^2}{4R^3G} + \frac{2\mu C}{G} \left( a^2 + \frac{2b^5}{3a^3} \right) \right) \cos^2 \lambda \cos 2(\theta - \mu t). \end{aligned} \right\}. \quad (\epsilon)$$

If we give to  $p$  any arbitrary value  $p_1$ , the equation ( $\delta$ ) will apply to a varying surface at all points of which the pressure is  $p_1$  at any time  $t$ . By obtaining  $(\delta p_1)$ , that is, by making  $p_1$  vary with respect to coordinates only, we should get the change of  $p_1$  in passing from one surface of equal pressure to a conti-

guous one at a *given time*. The variation  $\left(\frac{dp_1}{dt}\right)\delta t$  would give the change of  $p_1$ , in passing to a contiguous surface of equal pressure, when the change due to variation of position of the surfaces in a given small time is taken into account. If the value of  $p_1$  be the same for the two contiguous surfaces, we shall have  $\left(\frac{dp_1}{dt}\right)=0$ . Now this is the case with respect to the exterior surface of the ocean, which is always a surface of equal pressure, and of the *same* pressure at all times, the pressure being that of the atmosphere. Consequently an equation for determining C should be obtained by equating to zero  $\left(\frac{dp_1}{dt}\right)$ , and putting  $a$  for  $r$  in the result.

In the expression for  $\left(\frac{dp_1}{dt}\right)$ , those terms containing  $\frac{dr}{dt}$ ,  $\frac{d\theta}{dt}$ , and  $\frac{d\lambda}{dt}$ , which would introduce terms of the second order with respect to  $m$ , are to be omitted. Hence

$$\left(\frac{dp_1}{dt}\right) = -G \frac{dr}{dt} + \left(\frac{3mr^2\mu}{2R^3} + 4\mu^2 C \left(r^2 + \frac{2b^5}{3r^3}\right)\right) \cos^2 \lambda \sin 2(\theta - \mu t).$$

Putting now for  $\frac{dr}{dt}$ , or  $\frac{d\phi}{dr}$ , its value

$$2C \left(r - \frac{b^5}{r^4}\right) \cos^2 \lambda \sin 2(\theta - \mu t),$$

substituting  $a$  for  $r$ , and equating the result to zero, we get the following equation for calculating C :

$$\frac{3ma^2\mu}{2R^3} = 2C \left\{ G \left(a - \frac{b^5}{a^4}\right) - 2\mu^2 \left(a^2 + \frac{2b^5}{3a^3}\right) \right\},$$

or

$$C = \frac{\frac{3ma\mu}{4R^3G}}{1 - \frac{b^5}{a^5} - \frac{2\mu^2 a}{G} \left(1 + \frac{2b^5}{3a^5}\right)}.$$

By numerical calculation made on the suppositions that  $a-b$ , the depth of the ocean, is 3 miles,  $a=3956$  miles,  $\mu = \frac{79}{82} \times$  the earth's rotation in one second,

$$\frac{a^2 a}{G} = \frac{1}{311.4}, \quad \frac{ma}{GR^3} = \frac{m}{E} \times \frac{a^3}{R^3} = \frac{1}{70} \times \frac{1}{(60.3)^3},$$



it will be found that  $C = -0.0000070819\mu$ , the attracting body being the moon.

If  $r_1$  be the value of  $r$  where  $\theta - \mu t = 0$ , and  $r_2$  its value where  $\theta - \mu t = \frac{\pi}{2}$ , and if  $\lambda = 0$ , we shall get by integrating the equation

$$\frac{dr}{dt} = 2Ca \left(1 - \frac{b^5}{a^5}\right) \sin 2(\theta - \mu t),$$

or by inference from the equation ( $\epsilon$ ),

$$r_1 - r_2 = \frac{2Ca}{\mu} \left(1 - \frac{b^5}{a^5}\right).$$

By this formula the numerical value of  $r_1 - r_2$  is  $-1.12056$  ft.

Also the value of  $\frac{3ma^2}{4GR^3}$  is found to be  $1.02071$  ft. Consequently the equation of the ocean-surface is

$$\begin{aligned} r &= a + 1.02071 \cos^2 \lambda - 0.56028 \cos^2 \lambda \cos 2(\theta - \mu t) \\ &= a + 1.58099 \cos^2 \lambda - 1.12056 \cos^2 \lambda \cos^2 (\theta - \mu t), \end{aligned}$$

the numerical coefficients being expressed in feet. Hence it follows that the tide consists of two parts—one of which is the same for all longitudes, and varies as the square of the cosine of the latitude, and the other varies as the square of the cosine of the distance of any position from the point to which the moon is vertical. So far as the form of the ocean-surface depends on the latter part, it is that of an *oblate* spheroid the axis of which always passes through the position of the moon, *low water being under the moon*.

Now, although this solution satisfies all the hydrodynamical equations as well as the given conditions of the problem, there is a circumstance to be taken into account which shows that it cannot be the true solution. It will be seen that for a certain value of  $b$  the denominator of the expression for  $C$  might become zero, in which case this quantity and the vertical velocity would be infinite. This inference is indicative of a breach of continuity in the formula, to which there can be nothing corresponding in the movement of the fluid. The value of  $\frac{b}{a}$  which satisfies the equation

$$1 - \frac{b^5}{a^5} = \frac{2\mu^2 a}{G} \left(1 + \frac{2b^5}{3a^5}\right)$$

being found to be  $0.997879$ , it follows that

$$a - b = a \times 0.002121 = 8.47 \text{ miles.}$$

Accordingly, if the depth of the ocean exceed  $8\frac{1}{2}$  miles, the maximum height of the tide passes through an infinite value, the

directions of the tidal motions are reversed, and the low water under the moon is changed to high water. But clearly these conclusions are wholly inadmissible.

I take occasion here to state that the above solution appears to be of the same character as that given by the Astronomer Royal in the 'Monthly Notices of the Royal Astronomical Society' (April 13, 1866), to which I adverted in my former article in the January Number. He finds that under the moon it is low water "unless the depth of the sea exceed 12 miles," the denominator of the expression for the tidal elevation vanishing for this particular depth. The difference between 12 miles and  $8\frac{1}{2}$  miles is probably to be accounted for by the circumstance that the former depth applies to an "equatorial canal," whereas the other was obtained without any such restriction. To find that the tidal elevation would be indefinitely great if the ocean had a depth so small as 12 miles compared with the earth's radius, is a fatal difficulty, indicating failure of the reasoning, and not in any degree got over by saying that this depth "far exceeds any supposed real depth of the sea."

When in consequence of the above-mentioned results I had almost despaired of being able to discover the right method of treating this problem, it occurred to me to adopt the process of reasoning I now proceed to explain. On the principle announced in the former article, that any general integral of the equation ( $\alpha$ ) which can be obtained prior to the consideration of a given case of motion (such as the integral  $V = \frac{\phi(t)}{r^{1/2}}$ ) has, to a particular solution applying to given circumstances of the motion, the same kind of relation as that of the general integral to the particular solution of a differential equation between two variables, it appears that such particular solution, instead of satisfying the equation ( $\alpha$ ), will satisfy a *variation* of this equation obtained by giving indefinitely small increments to the coordinates. The reason for this assertion is, that the particular solution coincides with the general integral at a given time only *through an indefinitely small space*. In fact, unless this principle be true, it seems hardly possible to account for the failure of the solution above tried, which satisfied the equation ( $\alpha$ ) together with all the given conditions.

For conducting the proposed course of reasoning it will now be convenient to employ, in place of the equation ( $\alpha$ ), the equivalent one obtained by transforming the rectangular coordinates  $x, y, z$  into the polar coordinates  $r, \theta, \lambda$ . When this is done according to the usual rules, the result is

$$\frac{d^2 r \phi}{dr^2} + \frac{1}{r^2 \cos^2 \lambda} \frac{d^2 r \phi}{d\theta^2} + \frac{1}{r^2} \frac{d^2 r \phi}{d\lambda^2} - \frac{\tan \lambda}{r^2} \frac{d r \phi}{d\lambda} = 0. \quad (\theta)$$

This equation being represented, for the sake of brevity, as  $\Phi=0$ , according to the principle above stated the value of  $\phi$  which applies to the given circumstances of the problem is required to satisfy the equation

$$\frac{d\Phi}{dr} \delta r + \frac{d\Phi}{d\theta} \delta \theta + \frac{d\Phi}{d\lambda} \delta \lambda = 0.$$

Since under the given conditions  $\delta r$ ,  $\delta \theta$ ,  $\delta \lambda$  may be considered to be independent variations, this equation is equivalent to the three

$$\frac{d\Phi}{dr} = 0, \quad \frac{d\Phi}{d\theta} = 0, \quad \frac{d\Phi}{d\lambda} = 0.$$

It will now be supposed, as before, that

$$\phi = F(r) \cos^2 \lambda \sin 2(\theta - \mu t),$$

this being not a *hypothetical* form of the function, but derivable from the given conditions of the problem, as the following argument will show. Resuming the equation ( $\gamma$ ), differentiating it completely with respect to  $t$ , and omitting terms of a higher order than the first, the result is

$$\left(\frac{dp}{dt}\right) = -G \frac{dr}{dt} + \frac{3mr^2\mu}{2R^3} \cos^2 \lambda \sin 2(\theta - \mu t) - \frac{d^2\phi}{dt^2} + \psi'(t).$$

Supposing that  $\phi = \phi' + \int \psi(t) dt$ , we shall have  $\frac{d^2\phi}{dt^2} = \frac{d^2\phi'}{dt^2} + \psi'(t)$ ,

and  $\frac{dr}{dt} = \frac{d\phi}{dr} = \frac{d\phi'}{dr}$ ; and for the case in which this equation ap-

plies to the upper surface of the fluid, so that  $\left(\frac{dp}{dt}\right) = 0$ , we may put  $a$  for  $r$  in the terms of the first order with respect to  $m$ . Let  $\frac{d\phi'_0}{dr}$  and  $\frac{d^2\phi'_0}{dt^2}$  represent what  $\frac{d\phi'}{dr}$  and  $\frac{d^2\phi'}{dt^2}$  become by this substitution. Then we have

$$0 = -G \frac{d\phi'_0}{dr} + \frac{3ma^2\mu}{2R^3} \cos^2 \lambda \sin 2(\theta - \mu t) - \frac{d^2\phi'_0}{dt^2}.$$

Now this equation must be satisfied independently of particular values of  $\lambda$ ,  $\theta$ , and  $t$ ; which condition is fulfilled by the assumed value of  $\phi$  if  $\psi'(t) = 0$ , or  $\psi(t)$  be a constant, which, as we shall presently see, is the case. For then  $\cos^2 \lambda \sin 2(\theta - \mu t)$  is a factor of all the terms, so that the equation becomes

$$0 = -GF(a) + \frac{3ma^2\mu}{2R^3} + \mu^2 F(a),$$

and can be satisfied by an appropriate value of  $F(a)$ . From this reasoning it follows that the assumed value of  $\phi$  accords with

the given circumstances of the surface of the fluid; and if it be found capable of satisfying all the other conditions, it must give the true solution of the problem, because, from the nature of the case, the solution must be of a unique and definite character.

Putting  $f(r)$  for  $rF(r)$ , we have

$$r\phi = f(r) \cos^2 \lambda \sin 2(\theta - \mu t).$$

The substitution of this value in the left-hand side of the equation ( $\theta$ ) gives the result

$$\left( rf''(r) - \frac{6f(r)}{r} \right) \cos^2 \lambda \sin 2(\theta - \mu t),$$

which, for the present instance, is the quantity I have called  $\Phi$ . We have seen that  $\Phi = 0$  does not give an appropriate solution.

Also  $\frac{d\Phi}{d\theta} = 0$  and  $\frac{d\Phi}{d\lambda} = 0$  are inapplicable to the inquiry, because

$\theta$  and  $\lambda$  are contained in an explicit manner in the above expression. But the equation  $\frac{d\Phi}{dr} = 0$  becomes

$$\left( f'''(r) + \frac{f''(r)}{r} - \frac{6f'(r)}{r^2} + \frac{6f(r)}{r^3} \right) \cos^2 \lambda \sin 2(\theta - \mu t) = 0,$$

which is proper for finding the form of  $f(r)$  by integration. By assuming that  $f(r) = cr^m$ , there results for finding the values of  $m$  the equation

$$m(m-1)(m-2) + m(m-1) - 6m + 6 = 0.$$

Hence  $m$  has the three values 3, 1, and  $-2$ , and consequently

$$f(r) = c_1 r^3 + c_2 r + c_3 r^{-2},$$

$$\frac{f(r)}{r} = F(r) = c_1 r^2 + c_2 + c_3 r^{-3}.$$

The form of  $F(r)$  being thus found, the following equations immediately result:—

$$\phi = (c_1 r^2 + c_2 + c_3 r^{-3}) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$\frac{d\phi}{dt} = -2\mu(c_1 r^2 + c_2 + c_3 r^{-3}) \cos^2 \lambda \cos 2(\theta - \mu t),$$

$$\frac{d^2\phi}{dt^2} = -4\mu^2(c_1 r^2 + c_2 + c_3 r^{-3}) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$u = \frac{d\phi}{dr} = (2c_1 r - 3c_3 r^{-4}) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$v = \frac{1}{r \cos \lambda} \frac{d\phi}{d\theta} = 2(c_1 r + c_2 r^{-1} + c_3 r^{-4}) \cos \lambda \cos 2(\theta - \mu t),$$

$$w = \frac{1}{r} \frac{d\phi}{d\lambda} = -(c_1 r + c_2 r^{-1} + c_3 r^{-4}) \sin 2\lambda \sin 2(\theta - \mu t).$$



The radius of the lower surface of the ocean being  $b$ , the condition  $u=0$  for this surface gives

$$2c_1b - 3c_3b^{-4} = 0, \text{ or } \frac{c_3}{c_1} = \frac{2b^5}{3}.$$

From the above values of  $\frac{d\phi}{dr}$  and  $\frac{d\phi}{dt}$  it may be shown, just as in the previous investigation, that  $\psi(t) = \varpi + Ga + \frac{ma^2}{2R^3}$ , and is therefore a constant.

Again, by substitution in the equation obtained by differentiating ( $\gamma$ ) completely with respect to  $t$ , and equating the left-hand side to zero after putting  $a$  for  $r$  in the other side, there results

$$0 = -2Gc_1\left(a - \frac{b^5}{a^4}\right) + \frac{3ma^2\mu}{2R^3} + 4\mu^2c_1\left(a^2 + \frac{c_2}{c_1} + \frac{2b^5}{2a^3}\right).$$

Hence

$$c_1 = \frac{\frac{3ma\mu}{4GR^3} + \frac{2\mu^2c_2}{Ga}}{1 - \frac{b^5}{a^5} - \frac{2\mu^2a}{G}\left(1 + \frac{2b^5}{3a^5}\right)}.$$

Let  $b_0$  be the value of  $b$  which causes the denominator of this fraction to vanish. Then, putting  $\omega$  for  $\frac{\mu^2a}{G}$ , it will be found that this denominator is equal to

$$(1 - 2\omega)\left(1 - \frac{b^5}{b_0^5}\right).$$

We may now employ the arbitrary quantity  $c_2$  to get rid of the factor  $1 - \frac{b}{b_0}$ , by which the result would otherwise be vitiated. It is clear that, since for this purpose  $c_2$  must be negative, we must assume that

$$\frac{3ma\mu}{4GR^3} + \frac{2\mu^2c_2}{Ga} = \frac{3ma\mu}{4GR^3}\left(1 - \frac{b}{b_0}\right);$$

for if we put  $C_0\left(1 - \frac{b}{b_0}\right)$  in the place of the simple factor  $1 - \frac{b}{b_0}$ ,  $C_0$  being any arbitrary positive quantity, the two sides of the above equality could not be identical. Hence it follows that

$$c_1 = \frac{\frac{3ma\mu}{4GR^3}}{(1 - 2\omega)\left(1 + \frac{b}{b_0} + \frac{b^2}{b_0^2} + \frac{b^3}{b_0^3} + \frac{b^4}{b_0^4}\right)}.$$

We have thus determined the value of  $c_1$  and proved that it is always *positive*. The value of  $c_3$  is given by the equation

$c_3 = -\frac{2b^5c_1}{3}$ ; and for calculating  $c_2$  we have

$$\frac{2\mu^2c_2}{Ga} = -\frac{3ma\mu b}{4GR^3b_0}; \text{ or } c_2 = -\frac{3ma^2b}{8R^3\mu b_0}.$$

By employing the value now obtained for  $\frac{d\phi}{dt}$ , we shall have, since

$$\frac{c_1a}{\mu}\left(1 - \frac{b^5}{a^5}\right) = \frac{3ma^2}{4GR^3} + \frac{2mc_1}{G}\left(a^2 + \frac{c_2}{c_1} + \frac{2b^5}{3a^3}\right), \quad . \quad . \quad (\kappa)$$

for the equation of the upper surface of the water

$$r = a + \frac{3ma^2}{4R^3G} \cos^2 \lambda + \frac{c_1a}{\mu} \left(1 - \frac{b^5}{a^5}\right) \cos^2 \lambda \cos 2(\theta - \mu t) = a + \left(\frac{3ma^2}{4R^3G} - \frac{c_1a}{\mu} \left(1 - \frac{b^5}{a^5}\right)\right) \cos^2 \lambda + \frac{2c_1a}{\mu} \left(1 - \frac{b^5}{a^5}\right) \cos^2 \lambda \cos^2(\theta - \mu t).$$

So far as the first and last terms of the second form of the equation indicate, the surface, since  $c_1$  is positive, is that of a *prolate* spheroid, the axis of which is always directed to the place of the attracting body. The other term gives the amount of deviation from this form.

The first form of the equation shows that there is a constant elevation of the waters, varying when the body is in the equator, as the square of the cosine of latitude, and that the maximum variation from this elevation, or difference between high and low water at a place whose latitude is  $\lambda$ , is

$$\frac{2c_1a}{\mu} \left(1 - \frac{b^5}{a^5}\right) \cos^2 \lambda.$$

This formula may be tested by applying it to the case in which  $\mu=0$ , that is, the case of equilibrium of the waters, for which, as is known,

$$r = a + \frac{3ma^2}{2GR^3} \cos^2 \lambda \cos^2(\theta - \mu t).$$

Now this equation results from the second of the above expressions for  $r$  by supposing that

$$\frac{c_1a}{\mu} \left(1 - \frac{b^5}{a^5}\right) = \frac{3ma^2}{4GR^3};$$

whence it follows that in the case of equilibrium the difference between the greatest and least values of  $r$  is  $\frac{3ma^2}{2GR^3} \cos^2 \lambda$ , as is

also evident from the above value of  $r$ . At the same time, by reason of the equation ( $\kappa$ ), we have

$$\frac{2\mu c_1 a^2}{G} \left(1 + \frac{2b^5}{3a^5}\right) + \frac{2\mu c_2}{G} = 0.$$

The expression for  $c_1$  shows that the first term of this equation vanishes if  $\mu=0$ ; that the other vanishes in the same case may be thus proved. Since

$$\frac{3ma\mu}{4GR^3} \left(1 - \frac{b}{b_0}\right) = c_1 \left(1 - \frac{b^5}{a^5} - \frac{2\mu^2 a}{G} \left(1 + \frac{2b^5}{3a^5}\right)\right),$$

and from the foregoing supposition

$$\frac{3ma\mu}{4GR^3} = c_1 \left(1 - \frac{b^5}{a^5}\right),$$

it follows that

$$\frac{3mb}{4GR^3 b_0} = \frac{2\mu c_1}{G} \left(1 + \frac{2b^5}{a^5}\right).$$

Hence, since the value of  $c_1$  shows that  $\mu c_1$  is equal to  $\mu^2 \times$  a positive factor, the value of  $\frac{b}{b_0}$  has this form also. It thus appears from the value of  $c_2$  that not only is the foregoing equation verified when  $\mu=0$ , but we have also  $c_2=0$ . Consequently the expressions obtained for  $u$ ,  $v$ , and  $w$  severally vanish, as plainly should happen in the case of equilibrium.

I proceed now to calculate the numerical values of the coefficients in the expressions for height of tide and the vertical and horizontal velocities, the attracting body being supposed to be the moon, and the depth of the ocean three miles. From the formula which gives the value of  $c_1$  we have

$$\frac{c_1 a}{\mu} \left(1 - \frac{b^5}{a^5}\right) = \frac{\frac{3ma^2}{4GR^3} \left(1 - \frac{b^5}{a^5}\right) \left(1 - \frac{b}{b_0}\right)}{(1-2\omega) \left(1 - \frac{b^5}{b_0^5}\right)}.$$

Hence, since by previous calculation  $\frac{3ma^2}{4R^3 G} = 1.02071$  foot,

$\omega = \frac{1}{311.4}$ ,  $1 - \frac{b^5}{a^5} = 0.003786$ ,  $\frac{b}{b_0} = 1.001386$ , the left-hand side of the above equation will be found to be equal to 0.000776 foot. Accordingly the maximum difference of the height of the waters is 0.001552 ft., and the equation of the exterior surface is

$$r = a + 1.02071 \cos^2 \lambda + 0.000776 \cos^2 \lambda \cos 2(\theta - \mu t),$$

or

$$r = a + 1.01993 \cos^2 \lambda + 0.001552 \cos^2 \lambda \cos^2(\theta - \mu t).$$

We have also

$$\frac{2\mu c_2}{G} = -\frac{3ma^2}{4R^3G} \times \frac{b}{b_0} = -1.02212 \text{ ft.}$$

Hence it will be found, by calculating with the data thus obtained, that  $2c_1a\left(1 - \frac{b^5}{a^5}\right) = 0.000000109 \text{ ft.}$ , and

$$2c_1a\left(1 + \frac{c_2}{a^2c_1} + \frac{c_3}{a^5c_1}\right) = -0.022252 \text{ ft.}$$

Consequently the velocities at the ocean-surface have the following values :—

$$\begin{aligned} u &= +0.000000109 \cos^2 \lambda \sin 2(\theta - \mu t), \\ v &= -0.022252 \cos \lambda \cos 2(\theta - \mu t), \\ w &= +0.011126 \sin 2\lambda \sin 2(\theta - \mu t). \end{aligned}$$

What is chiefly noticeable in these results is the small amount of the change of elevation of the water at a given position. It is even less than that which resulted from my first attempt, which I thought to be conclusive against it. It now, however, appears to me that this objection was not valid, and that the error of that method consisted solely in not making use of the equation ( $\alpha$ ). As this equation is taken into account in the present method, I can perceive no reason why the results should not be admissible. The small tide is consistent with the values of the velocities  $v$  and  $w$ , the latter in particular tending always to diffuse the elevation or depression of the waters over a larger area. The mode of action conceived of by Newton in Props. 36 and 37 of Book III. is in accordance with this theory, especially if the ocean be of very great depth, the amount of tide being greater as  $b$  is less.

If the foregoing reasoning be good, the large amounts of tide observed on sea-shores must be produced by the obstacles which continents and islands present to the free horizontal movement of the waters, as well as by the contraction of the channel of the tidal stream by the configuration of the coasts, and in no small degree, it may be, by the inequalities of the bottom of the ocean. This view appears to receive confirmation from facts stated by Whewell in Series XIII. of his "Researches on the Tides" (Phil. Trans. for 1848), viz. that "the tides of islands in mid-ocean are very small" (p. 2); that there are "very small tides, or no tides, at the islands in the centre of the Pacific,



Tahiti and the Sandwich Islands" (p. 6) ; and that "the tides over a great portion of the Pacific are so small that we may consider the lunar tide as almost vanishing" (p. 18). It is evident that the conditions supposed in the theory are likely to be more approximately fulfilled at the central portion of this large ocean than at any other part of the earth's surface.

Further, it may be remarked that the principal term in the expression for  $\eta$ , which varies as  $\cos^2 \lambda$ , is constant for a given latitude only because the moon was supposed to be in the equator. If the problem were treated more generally, and the moon's declination were taken into account, it may be presumed that there would be a corresponding term varying with the declination ; and as the variation would be continuous, and the term would represent the principal amount of tidal elevation, the effect might be exhibited as a sensible *diurnal* variation of the tide. Whewell has, in fact, made mention, in the memoir above cited, of "a diurnal inequality following the changes of the moon's declination."

At the end of my communication on this problem in the January Number I said (as it now appears, on insufficient grounds) that the solution it contained was "strictly based on the necessary principles of hydrodynamics." My object in both attempts has been to point out a course of reasoning by which this great desideratum in the theoretical treatment of tides might be attained. The present mathematical theory, by clearing up a difficulty relating to the logical method of applying the general equation ( $\alpha$ ), has, I think, materially contributed towards accomplishing this object. The difficulty I refer to consisted in obtaining impossible results by applying that equation in a manner accordant with received hydrodynamical principles. As the proposed explanation of it introduces a new and important principle in the application of analysis to hydrodynamics, I have thought it worth while to call attention to the reasoning employed by stating it here again as succinctly as possible.

Since the general equation ( $\alpha$ ), or  $\Phi=0$ , is applicable to all parts of the fluid at all times, it would still be true if to given values of  $x, y, z$ , and  $t$  the small independent variations  $\delta x, \delta y, \delta z$ , and  $\delta t$  were added. That is, we shall have  $\Phi + \delta\Phi=0$ , as well as  $\Phi=0$ , for the *same* values of  $x, y, z$ , and  $t$ . Hence we have a new general equation  $\delta\Phi=0$ . The general integral of  $\Phi=0$ , obtained independently of any particular case of motion, must satisfy both  $\Phi=0$  and  $\delta\Phi=0$ . But a particular solution which applies to the circumstances of a given case of motion is only required to satisfy  $\delta\Phi=0$  because it coincides with the general integral only through an indefinitely small space for an indefinitely small time; and if it also satisfied the equation  $\Phi=0$ ,

it would not be distinguishable from a particular form of the general integral.

There would, I think, be no difficulty in extending the method of solution adopted in this communication so as to take into account the change of the moon's declination, and to include also terms of the second order. But I am not prepared to undertake the labour of going through the details of the analytical and numerical calculations which would be required.

Cambridge, March 11, 1870.

XXXV. *Contributions to the Mineralogy of Nova Scotia.* By Professor How, D.C.L., University of King's College, Windsor, Nova Scotia.

[Continued from vol. xxxvii. p. 271.]

V. *New Forms of the Borates in Gypsum—Crust of Glauber salt and Epsomite containing Boracic Acid on Gypsum—The Borates as Fluxes in Welding—Loss and Gain of Water by Varieties of Crystallized Gypsum.*

**N**ATROBOROCCALCITE in distinct Crystals.—Like many other minerals of complex composition, natroborocalcite, as I have called it in former papers (Ulexite of Dana\*, from the name of its first analyst), has engaged the attention of many inquirers, who have expressed different opinions as to the composition of the species. In addition to the fourteen analyses given by Dana, who furnishes a ratio from the results of Rammelsberg and one from mine, others have been published, from which discordant conclusions have been drawn. With regard to Rammelsberg's formula, Dr. Lunge† says "it cannot be denied that the percentage of boracic acid found falls much too short of that required by his formula, and agrees better with that of Kraut." This formula, attributed to Kraut, is really mine by priority, as not overlooked by that gentleman; but it received his name from Dr. Lunge because of his endeavours to show its general validity from many other analyses than mine‡. When the circumstances under which the mineral occurs are reviewed, the reason of the want of agreement among analysts is manifest. The most abundant supply is obtained from Southern Peru, where, known as tiza, it is in the closest association with nitrate of soda, Pickeringite, Glauberite, salt, gypsum, chloride of calcium, and other substances; and specimens from

\* Mineralogy. Fifth edition, p. 598.

† Chemical News, vol. xv. p. 86.

‡ Loc. cit. p. 214.

this region have been used for the greater number of known analyses. I have seen no mention of the mode of occurrence in Western Africa; but salt is given as an ingredient there. Nevada affords it in a salt-marsh—in layers alternating with salt, and in balls in the salt. In Nova Scotia we have it associated occasionally with Glauber salt, calcite or Arragonite, and selenite, but most frequently imbedded in gypsum in what have hitherto been known as its only form, viz. nodules or rounded balls of closely packed fibres, which are capillary or acicular crystals. In the analysis made of the specimens first noticed here I found very little impurity, consisting of a small quantity of sulphate of sodium and of magnesium and a trace of chlorine, which was readily removed by cold water, so that a pure material was without difficulty obtained for examination; and I was led to propose the formula which Dr. Kraut subsequently preferred as expressing most satisfactorily, in view of all the published analyses, the composition of the mineral, and which, as shown in a former part of these Contributions\*, does not appear to me to be set aside by his later results and those of Dr. Lunge, —an opinion evidently shared with Dana, who has not, in the last edition of his ‘Mineralogy’ (*loc. cit.*), accepted the conclusions of the last-named chemist. It was with reference to the varying formulæ given from time to time that Mr. Walker, a late observer of the conditions under which the “so-called borate of lime” is found in Peru, felt himself called upon in 1868 to select seven specimens representing so many distinct deposits occurring in different basins, and to show, by analysis†, that they were most dissimilar admixtures of various substances. For these he (doubtless with great self-restraint) would not attempt to deduce a formula; and in this forbearance he offered an example to those who, as he said, “seemed to delight in racking their brains to construct a formula that will agree with their particular analysis.” It is obvious that the analyses given by Dana are those of a definite substance, varying, however, in purity, and that the Nova-Scotian specimen is the purest, with one exception, of those mentioned as actually found; so that Mr. Walker’s seven carefully selected *muds* really do not militate against the existence of this well-marked and interesting species.

Last year, the gypsum trade being unusually active here, not less than about 106,000 tons having been shipped from Hants county, and of this 81,276 tons from Windsor, in great part from its own wharfs, as against 100,159 and 63,655 tons respec-

\* Phil. Mag. January 1868.

† Chemical News, vol. xviii. p. 203.

tively, the largest known previous annual exportation\*, I had an excellent opportunity of examining varieties of the rock from many quarries, and I became acquainted with additional localities for its accessory minerals. One place in Newport, about six miles from Windsor, furnished specimens of natroborocalcite of an entirely novel character. Besides the compact hard nodules formerly noticed†, in which structure was scarcely apparent to the naked eye, imbedded in solid gypsum, others, of looser texture, were found in small cavities of the same rock made up of tufts of needles pressed close together at the centre and radiating out in divergent needles, while others were formed of crystals separate almost to the common central point of attachment. Larger cavities exhibited isolated delicate prismatic crystals half an inch long or more; these were perfectly colourless and transparent, and appeared under the microscope to be four-sided (and possibly square) prisms. The only associate was selenite, of which equally colourless and transparent crystals were not unfrequently observed overlying the borate in tufts, and also standing up among its isolated prisms. The specimens of natroborocalcite now described are not nearly so frequently found as the well-known nodules; the examination of many hundreds of tons of gypsum did not furnish by any means a large number of good ones; and they do not appear to be present in any other rock than that from one particular locality in Newport. They have a remarkably close resemblance to some forms of "needle-stone" found in trap, and, when freshly exposed, they are very beautiful in contrast with the old form: they require careful handling for the preservation of their characteristic appearance.

There are other interesting circumstances connected with recent observations of this borate. Apart from the crystals just mentioned, there is a close resemblance in the forms met with at different localities, which are now known to be several within the area of a few square miles.

In addition to the forms already mentioned, flattened masses are sometimes found, nearly an inch thick and three or four inches across, affording most brilliant surfaces when broken. Still flatter and much smaller masses, mere splashes, as it were, are occasionally seen dotted over some two or three square feet of grey translucent selenite; as this is seen covering with a thin layer whole sides of "stones" of quarried gypsum, these surfaces are probably very considerable in the bed; these splashes are so thickly strewn as scarcely to leave a square inch of selenite uncovered. The nodules present curious parallelisms in their

\* *Mineralogy of Nova Scotia*, p. 132.

† *Contributions*, III., *Phil. Mag.* January 1868.



mode of occurrence with the borate and chloride of magnesium, called boracite, found at Stassfurt. When they occur with the salts of sodium before specified, they exhibit the position of an insoluble borate associated with soluble salts occupied by boracite in carnallite and other chlorides at Stassfurt. The distribution of the nodules of Ulexite in gypsum in this vicinity is described exactly in the words used\* with reference to the magnesian borate, viz., "the nodules of boracite have the appearance of having been shot at random from a park of artillery; so irregularly are they scattered and so firmly are they imbedded." Further, the latter is also found in beds of anhydrite and gypsum; and though Ulexite has not been met with here in anhydrite, I have seen it in a very thin vein of gypsum in that rock. It is important to remember in this connexion that boracite is anhydrous, while Ulexite is largely hydrated.

*Silicoborocalcite in Crystalline Nodules.*—Recent observations have also added to the mineralogical history of this species, *Howlite* of Dana†. A pupil of mine, Mr. Keating, brought me last May a nodule made up of pearly scales, which proved to be the mineral in question. I subsequently obtained several specimens myself, and have quite recently had others, some being larger than any previously seen by me, and the largest weighing about two ounces, sent me from the same place. I had already on one occasion noticed, as mentioned in my original description of the species‡, a single nodule of similar characters from a locality in Newport, the usual forms (found elsewhere) having been at the same time described as dull and amorphous. Those lately met with were from Winkworth, about two miles from Windsor; they were imbedded in gypsum, which also contained Ulexite, most frequently alone, but sometimes underlying Howlite; the nodules were of irregular form, well rounded, and, when removed, left a smooth cavity in the rock; they were seamed with gypsum and selenite in some instances, like the amorphous nodules originally found: none of these were met with in the Winkworth gypsum. The pearly scales were perfectly transparent under the microscope, and consisted apparently of plates breaking with oblique cleavage-planes. In one form or other Howlite has now been met with, always in gypsum or anhydrite, at four localities lying from two to thirty miles apart, and reported from two others, all in Hants county.

*Incrustation of Gypsum by Glauber salt and Epsomite containing Boracic Acid.*—Clifton Quarry, Windsor, mentioned by me

\* "The Salt Deposits at Stassfurt," Chemical News, vol. xix. p. 78.

† Mineralogy. Fifth edition, p. 598.

‡ Phil. Mag. January 1868.

in 1857\* as affording the specimens of crystals of Glauber salt with Ulexite, then for the first time made known to occur in this province, has since undergone great changes from extensive workings in various parts. Last autumn, at a depth of 30 feet below the original surface, and from 2 to 5 feet below a level which had been a surface for two or three years and is now covered with dirt, a road was made through solid gypsum; and on both sides of this road was observed a crust covering a considerable area of the rock. When first exposed it was transparent, and it became opaque and powdery on exposure to the dry air; there were also observed here and there tufts of small prismatic crystals. The crust consisted essentially of Glauber salt; an analysis of some transparent portions, left a night only in the laboratory, gave—

Water . . . . .	: . . . .	54.10
Sulphate of soda and a little impurity . . . . .		45.90
		<hr/> 100.00

Pure Glauber salt contains 56 per cent. of water; the impurity noticed consisted of sulphate of lime and magnesia and perhaps chloride of sodium, as a trace of chlorine was found in some of the crust. The tufts of needles were sulphate of magnesia. It is a most interesting fact that the crust contained a minute quantity of boracic acid; several trials with turmeric paper in the regular way brought out a distinct reaction, not on a single dipping and drying, but on repeating this process once or more. With regard to magnesia, I have found it a pretty constant ingredient in our gypsum, sometimes to a notable amount; I have also proved the existence of probably a considerable percentage in limestone in contact with gypsum at one place.

*The Borates as Fluxes in Welding.*—As was to be expected from the fusibility of Ulexite and Howlite, they have both been found most perfect substitutes for borax in welding. The joint of a piece of hard cast steel welded with a specimen of Howlite proved perfect when the edges were beaten with a sledge-hammer; and both steel and iron have been welded with Ulexite.

*Variable loss of Water by Forms of Crystallized Gypsum on Heating.*—In the course of researches on gypsum I have become acquainted with some curious facts respecting the loss of water on heating its varieties. I found that when a specimen of fibrous gypsum and one of tabular selenite were heated side by side in an air-bath, the latter began to lose water at 190° F.; in about thirty minutes it had lost 0.94 per cent.; as the temperature rose, during three hours, continued loss ensued, till it amounted to 6.39 per cent. at 209° F., while the fibrous gypsum remained

\* Silliman's Journal, September 1857, p. 230.

unchanged in weight. At  $212^{\circ}$  F. tabular selenite lost in about an hour 2.41 per cent.; fibrous gypsum lost nothing the first half hour, and only 0.55 per cent. during about an hour. On continued heating at this temperature loss increased rapidly in both cases and became pretty uniform; at least the tabular lost in about six hours 14.1 per cent., while the fibrous lost from 12 to 15 per cent. in different specimens. On heating at a higher temperature, the total loss was 20.77 by the tabular and 20.82 by one specimen of fibrous gypsum, both numbers which agree with 20.88, the percentage of water in pure gypsum.

*Regaining of Water by Dried Gypsum.*—Tabular gypsum which had lost 20.77 per cent. water regained in about two days, when left in a room, 5.47 per cent. of its weight; fibrous, which had lost 20.71 per cent., regained in about thirty-six hours only 0.02 per cent.; fibrous which had lost 20.21 per cent. regained in two days 3.97 per cent.; and exposure for three hours at an open window on a damp October day gave scarcely any perceptible increase in either of these two cases. Fibrous gypsum which had lost 5.37 per cent. at  $212^{\circ}$  F. regained only .07 per cent. of its weight during exposure for some days in a room, and for three hours of the time to the damp open air. Hence it appears that tabular selenite both loses water more readily than fibrous gypsum, and regains more quickly that which has been expelled by moderate heat. What light these and other results, obtained by experiments of an allied nature, may throw on the mutual relations of gypsum and anhydrite, and the supposed derivation of the former from the latter, before touched upon\*, remains to be seen.

Windsor, Nova Scotia.

February 24, 1870.

### XXXVI. *On the Polarization of Heat.*

*By* PROFESSOR TYNDALL, *F.R.S., &c.*†

IN the Philosophical Magazine for November 1835 the late Principal Forbes gave an account of the experiments by which he demonstrated the polarization of non-luminous heat. He first operated with tourmalines, and afterwards, by a happy inspiration, devised piles of mica plates, which from their greater power of transmission enabled him more readily and conclusively to establish the fact of polarization. The subject was subsequently followed up by Melloni and other philosophers. With great sagacity Melloni turned to account his own discovery, that the ob-

\* Contributions, III., Phil. Mag. January 1868.

† Communicated by the Author.

seure rays of luminous sources were in part transmitted by black glass. Intercepting by a plate of this glass the light emitted by his oil lamp and operating upon the transmitted heat, he obtained effects exceeding in magnitude any that could be obtained by means of the radiation from obscure sources. The possession of a more perfect ray-filter and a more powerful source of heat enables us now to obtain, on a greatly augmented scale, the effects obtained by Forbes and Melloni.

Two large Nicol's prisms, such as those employed in my experiments on the polarization of light by nebulous matter, were placed in front of an electric lamp, and so supported that either of them could be turned round its horizontal axis. The beam from the lamp, rendered slightly convergent by the camera-lens, was sent through both prisms. But between them was placed a cell containing iodine dissolved in bisulphide of carbon in quantity sufficient to quench the strongest solar light. Behind the prisms was placed a thermo-electric pile, furnished with two conical reflectors. The hinder face of the pile received heat from a platinum spiral through which passed an electric current regulated by a rheostat.

The apparatus was so arranged that, when the principal sections of the Nicols were crossed, the needle of the galvanometer connected with the pile showed a deflection of  $90^\circ$  in favour of the posterior source of heat. One of the prisms was then turned so as to render the principal sections parallel. The needle immediately descended to zero, and passed on to  $90^\circ$  at the other side of it. Reversing, or continuing the motion, so as to render the principal sections again perpendicular to each other, the calorific sheaf was intercepted, the needle descended to zero and went up to its first position.

So copious indeed is the flow of polarized heat that a prompt rotation of the Nicol would cause the needle to spin several times round over its graduated dial.

These experiments were made with the delicate galvanometer employed in my researches upon radiant heat. But the action is so strong as to cause a coarse lecture-room galvanometer, with needles 6 inches long and paper indexes a square inch each in area, to move through an arc of nearly  $180^\circ$ .

Reflection, refraction, dispersion, polarization (plane and circular), double refraction, the formation of invisible images both by mirrors and lenses, may all be strikingly illustrated by the employment of the iodine filter and the electric light.

Take, for example, the following experiments:—The Nicols being crossed, the needle of the galvanometer pointed to  $78^\circ$  in favour of the heated platinum spiral behind the pile. A plate of mica was then placed across the dark beam with its prin-



cial section inclined at an angle of  $45^\circ$  to those of the Nicols. The needle instantly fell to zero, and went up to  $90^\circ$  on the other side.

And, for circular polarization:—The Nicols being crossed and the needle pointing to  $80^\circ$  in favour of the platinum spiral, a plate of rock-crystal cut perpendicular to the axis was placed across the dark beam. The needle fell to zero, and went to  $90^\circ$  on the other side.

The penetrative power of the heat here employed may be inferred from the fact that it traversed about 12 inches of Iceland spar, and about  $1\frac{1}{2}$  inch of the cell containing the solution of iodine.

Royal Institution, March 17, 1870.

XXXVII. On  $\sqrt{-1}$ . By FRANCIS GUTHRIE, LL.B., Professor of Mathematics, Graaff Reinet College, South Africa\*.

MATHEMATICIANS will probably agree that, whereas  $\sqrt{-1}$ , or some equivalent for it, can hardly be dispensed with in practice, the justifications hitherto given for the use of that symbol are extremely unsatisfactory. Double algebra indeed supplies a complete and most ingenious solution of the difficulty, but one which cannot but be considered as being too laborious and (involving, as it does, conceptions of space and direction) as somewhat inappropriate in a question of pure algebra. The endeavour to put the laws of abstract magnitude on a more simple elementary footing has led me to the following way of considering this subject, which I am desirous of submitting to those who take an interest in these matters.

The first notion of a power is arrived at in arithmetic as the abbreviation of the product of a number of equal numerical factors, the root being defined as the inverse of the power—a definition which, it may be observed, is preserved here and elsewhere, whatever extensions are afterwards given to the word power.

The first of these extensions results from the larger meaning which it is found convenient to give to the word multiplication, with a view to include negative factors. According to this definition, as  $x \times (+m)$  means  $x$  added  $m$  times, so  $x \times (-m)$  means  $x$  subtracted  $m$  times, and  $x \times (-m)^2$  or  $x \times (-m) \times (-m)$  means  $x \times (-m)$  subtracted  $m$  times, and so on. It is on account of this extension of the words multiplication and power that the inverse function (the root) gives rise to ambiguity of sign, so that  $\sqrt{x^2} = \pm x$ .

\* Communicated by Frederick Guthrie, Professor of Physics, Royal School of Mines.

The next extension of the meaning of the word power, which it is necessary for our present purpose to consider, is that by which fractional indices are explained. According to the definition adopted for this purpose, every power may be considered as having for its index a numerical ratio as  $x^{\frac{p}{q}}$ ; and such a power is defined as being that ratio multiplication by which  $q$  times is equivalent to multiplication  $p$  times by  $x$ , the need for a separate definition of the word root being thereby dispensed with.

It is on account of this extension that an inconsistency first arises in the theory of exponents. Theoretically  $x^{\frac{p}{q}}$  should be an exact equivalent for  $x^{\frac{2p}{2q}}$ ; whereas if  $q$  be an odd number,  $x^{\frac{p}{q}}$  has but one sign, whereas  $x^{\frac{2p}{2q}}$  is ambiguous; so that it might be that the true answer to a question might appear in the form  $x^{\frac{2p}{2q}}$ , whereas the form  $x^{\frac{p}{q}}$  would be incorrect. Such an inconvenience can only be guarded against by a consideration of each particular case, contrary to the principle which should be arrived at in mathematical processes generally.

The next step is to give a meaning to the expression  $x^n$  when the index  $n$  is any ratio whatever, commensurable or incommensurable. This gives rise to the conception of the *exponential* function, of which, however, there may be several definitions. One definition is, that by  $x^n$  where  $n$  is any ratio we mean the function which satisfies certain functional equations, as that  $\phi(x, n) \times \phi(x, m) = \phi(x, m+n)$  &c. The other is, that by  $x^n$  where  $n$  is incommensurable, we mean the limit of  $x^m$  where  $m$  is a commensurable ratio, when  $m$  approaches to equality with  $n$ . The former or functional definition is the one we shall adopt. In either case we are compelled, for the sake of writing, to consider all powers as having ambiguous signs, so that  $x^1 = \pm x$ . This is not always made apparent; but that it really is the case may be seen from the allied logarithmic function, where we are compelled to consider  $\log_{10}(-10)$  as well as  $\log_{10} 10$  as being equal to unity. According to the exponential definition of a power, therefore,  $(-1)^{\frac{1}{2}}$  is by no means an impossible quantity, since  $(-1)^{\frac{1}{2}}$  and  $(+1)^{\frac{1}{2}}$  are both of them  $\pm 1$ . As, however, it would be inconvenient entirely to dispense with the ordinary algebraical definition of a power on account of its connexion with the process of multiplication, we have to use two different definitions of powers at the same time, one of which includes the other. To avoid the misunderstanding which might possibly arise, let us for the present distinguish these functions as follows. Let  $x^2$

stand for  $x \times x$ , and let  $\phi(x, 2)$  stand for the corresponding exponential function, so that  $\phi(x, 2) = \pm x^2$ . Let the corresponding inverse functions be denoted by  $\sqrt{x}$  and  $\phi(x, \frac{1}{2})$  respectively; so that whereas  $\sqrt{-x^2}$  is impossible,  $\phi(-x^2, \frac{1}{2}) = \pm x$ .

Now, whatever be our definition of a power, it is necessary for us to preserve the truth of the fundamental laws of exponents, and, among the rest, that  $\phi(n) \times \phi(m) = \phi(m+n)$ ; so that the following relations must be true:—

$$\begin{aligned}\phi(x, 1) &= \pm x, \\ \phi(x, 1)\phi(x, 1) &= \phi(x, 2), \\ \phi(x, 2) &= \pm x^2, \\ (\pm x) \times (\pm x) &= \pm x^2.\end{aligned}$$

Now this last equation is undoubtedly true if we read the signs crosswise, as  $(+x) \times (-x)$  or  $(-x) \times (+x)$ , as well as parallel, as  $(+x) \times (+x)$  and  $(-x) \times (-x)$ . Or if we read the signs parallel in both cases, but invert their position in the second case—since, if the signs are read parallel,  $(\pm x) \times (\pm x) = +x^2$ , and  $(\pm x) \times (\mp x) = -x^2$ ; so that  $\pm x$  is the square root of  $-x^2$  if the order of the ambiguous signs be reversed in the factors. This affords the suggestion by means of which it appears to me to be possible to dispense with the use of the *impossible* quantity  $\sqrt{-1}$ .

Let  $(\pm)x$  mean that the ambiguous base which is to be repeated in a power is to be repeated with its signs in the same order; but let  $(\overline{\pm})x$  mean that base which is to be repeated with its signs in alternated order, in all cases the signs having to be read parallel.

From this it will be seen that every exponential power and root is ambiguous in sign, that the square root of a magnitude with a positive sign is the ambiguity in which the signs of the factors of the square are to be taken in the same order, whereas the square root of the negative magnitude is the ambiguity in which the signs are to be taken in alternating position in the several factors, so that

$$(\pm x)^2 = x^2 \text{ and } (\overline{\pm} x)^2 = -x^2, \text{ or } (\pm 1)^2 = 1 \text{ and } (\overline{\pm} 1)^2 = -1.$$

Extending this idea, let us call any power of an ambiguity, in which the alternate signs are read together, the *alternating* power of the ambiguity.

Then we shall have

$$\begin{aligned} |\pm x &= \pm x \\ (|\pm x)^2 &= -x \\ (|\pm x)^3 &= \mp x \\ (|\pm x)^4 &= +x \\ \&c. \quad \&c. \end{aligned}$$

Or, since all ambiguities of sign may be considered as referring to a supposititious factor unity, and omitting such factors as always understood, we have

$$|\pm = \pm, \quad |\pm^2 = -, \quad |\pm^3 = \mp, \text{ and } |\pm^4 = +.$$

It will be easily seen that in this way the sign  $|\pm$  answers to the sign  $\sqrt{-1}$  as ordinarily used.

With these definitions, the fundamental laws of algebraical powers will be found to be true, viz.

$$x^m \times x^n = x^{m+n}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$(x^m)^n = x^{m \cdot n}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$x^m \cdot y^m = (xy)^m, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

certain conditions being observed,—and likewise the theorem that if  $a + |\pm b = c + |\pm d$ , then  $a = c$  and  $b = d$ ; for  $a + |\pm b = c + |\pm d$  must be taken as meaning  $a + b = c + d$ , and  $a - b = c - d$  when  $a = b$  and  $c = d$ . Whence every result ordinarily deduced by means of the symbol  $\sqrt{-1}$  can readily be demonstrated.

The other principal symbol to which we require to give a meaning is  $\epsilon^x$  when  $x$  is an ambiguity. The *meaning* we give to it is the expansion  $1 + x + \frac{x^2}{1^2} + \dots$ , which, when  $x$  is ambiguous (say  $y|\pm$ ), will be seen to be

$$\left(1 - \frac{x^2}{1^2} + \frac{x^4}{1^4} - \dots\right) + \left(x - \frac{x^3}{1^3} + \frac{x^5}{1^5} - \dots\right)|\pm.$$

The convergent expansions,

$$1 - \frac{x^2}{1^2} + \frac{x^4}{1^4} - \dots \text{ and } x - \frac{x^3}{1^3} + \frac{x^5}{1^5} - \dots,$$

are called  $\cos x$  and  $\sin x$  respectively; and the other properties of these functions are easily deduced from purely algebraical considerations.

But how, if this be the *conventional* meaning we attach to  $\epsilon^x|\pm$ , do we know that the laws of exponents are true of this function?



This we prove from a consideration of the equivalent expansions. Since when  $x$  and  $y$  are not ambiguous,  $\epsilon^x \times \epsilon^y = \epsilon^{x+y}$ , therefore

$$\left(1 + x + \frac{x^2}{L^2} + \dots\right) \times \left(1 + y + \frac{y^2}{L^2} + \dots\right) = 1 + (x + y) + \frac{(x + y)^2}{L^2} + \dots$$

But since the laws of  $x^n$  are the same whether the factors are ambiguous or not, therefore  $\epsilon^x \times \epsilon^y = \epsilon^{x+y}$  when  $x$  and  $y$  are ambiguous.

XXXVIII. *On the Action of the Alkali-metals on the Ethers of the Fatty Acids.* By J. ALFRED WANKLYN, Corresponding Member of the Royal Bavarian Academy\*.

THE remark with which Frankland and Duppa close their recent communication to the Royal Society† induces me to place on record the following statement.

In Löwig's *Organischen Chemie*, which is now an old book, there are several accounts of his and Weidmann's work on the action of potassium and sodium on different ethers of the fatty acids, and also on benzoic ether,—the papers from which these accounts are taken having been published in and about the year 1840.

The general drift of this work was that the alkali-metals form compounds from which alcohol is liberated by the action of water (*i. e.* ethylates), and complex salts of new acids derived from the fatty acid by a process of reduction. In particular the action of potassium on acetic ether was investigated, and the result arrived at that there is *no disengagement* of permanent gas, but that ethylate of potassium and the potash salt of a complex organic acid (a reduced acetic acid) is produced.

In this state the subject remained until comparatively recently, when it was investigated by Geuther, Greiner, Frankland, Duppa, and myself. It would not, however, be right to pass over a note, which is to be found under Acetic Ether in the English translation of Gmelin (vol. viii. p. 499), wherein the very happy suggestion is made that the reaction probably consists in the formation of *ethylate of potassium* and *acetylde of potassium*. With this solitary exception, no attempt either at explanation or at further investigation is recorded until the appearance of Geuther's paper in 1864.

Geuther investigated the salt of the complex organic acid, and deduced the formula  $C^6 H^9 NaO^3$  from his analyses, and prepared the hydrogen, ethyle, and methyle derivatives of it. He explained its generation from acetic ether and sodium by an equa-

\* Communicated by the Author.

† Page 309 of the present Number.

tion in which free hydrogen occurred, and asserted that disengagement of hydrogen took place during his experiment.

Frankland and Duppa confirmed Geuther's formula by preparing the ethyle derivative of the soda-salt, and asserted most explicitly the evolution of hydrogen. A marked feature of their investigation is the great prominence given to products of secondary action, and the mistaking of them for the primary products.

It was next pointed out by me that the formula  $C^6 H^9 NaO^3$  is equal to three atoms of acetylc and one atom of sodium. It was further shown by me that, during action of alkali-metals on acetic ether, one quarter of the metal enters into a new form of combination, and three quarters form ethylate of the metal. (This was shown both for potassium and for sodium.) It was also shown that, when proper care is taken to employ pure acetic ether and not to allow secondary products to be produced, there is, as Löwig had found in 1840, no disengagement of permanent gas.

These experimental data warrant, and are expressed by, this equation,



Although my demonstration of the non-evolution of hydrogen was published so long ago as the summer of 1868, and although I read the paper to Dr. Frankland, who was President of the Chemical Section of the British Association at the time, still no step was taken by Frankland and Duppa until February 1870, when the communication which occasioned the writing of this notice was made to the Royal Society.

From this communication I gather that Frankland and Duppa are not prepared to withdraw the account which they have published of the course of the action of sodium on acetic ether, and that they still maintain that changes do take place in which free hydrogen is directly evolved. Their original account, published in 1866, in which they wrote a number of equations, each one expressing an evolution of an equivalent of free hydrogen for every equivalent of sodium consumed, is, however, so far modified that they now appear to admit the occurrence of an accompanying reaction not involving free hydrogen. So far as I am able to understand their recent paper, they appear to say that at high pressures one kind of reaction takes place between sodium and acetic ether, and at low pressures another kind of action takes place. According to them, my reaction occurs at high pressures and theirs at low pressures.

However, none of my experiments were conducted at high pressures; some of them were at the ordinary atmospheric pressure; and I am at a loss to comprehend how such an explanation

can have been attempted. Moreover, the question whether or not hydrogen is a product of the action of sodium on acetic ether is a plain question of fact; and I will content myself with the simple statement, based on my experiments, that neither acetic ether nor any analogous ether evolves hydrogen by action on metallic sodium, and that every trace of that gas obtained by Frankland and Duppa was derived from alcohol and not from acetic ether. Sodium is now cheap, so is acetic ether; let those who desire other information make the experiment for themselves.

XXXIX. *On the Circumstances which determine the Variation of Temperature in a Perfect Gas during Expansion and Condensation.* By the Rev. J. M. HEATH\*.

THE constancy of the total energy (potential energy + actual) of a given weight of gas during any changes in its condition caused by the internal forces by which its particles act upon one another, depends upon two conditions—that the internal forces or energies do *all* the internal work that is done, and that they do *no other*. When these conditions do not hold—that is to say, when either external forces do any part of the internal work that is done, or when any part of the internal forces (potential energy) is spent upon work external to the system—then the total energy of the gas will be altered—by augmentation in the one case, by diminution in the other. When the denomination of the forces (external or internal) is the same as that of the work they do, the total energy of the same name is unaltered by their action. When the force is of a different name from the work it does, the total energies of both names are altered, but in opposite ways.

The internal forces of a gas consist of pairs of equal and opposite forces, by which each of two particles acts upon and is acted upon by the other. And as the external surface of the gas would consist of particles acted upon by the interior particles but not reacting upon them in return, the reaction of the vessel which bounds this surface in every part is necessarily included in the calculation of the sum of all the internal forces acting among the particles of the gas.

~ A gas, then, confined in a vessel and reacted upon at the surface of the vessel by a pressure equal and opposite to its own pressure upon the vessel, is to be taken as acted upon by internal forces and no other.

If, now, internal work is done in this gas subject to the condition last mentioned, that the reaction of the containing vessel

\* Communicated by the Author.

shall remain always strictly equal to the pressure of the elasticity of the gas, no external force will be introduced, and no external work will be done; and consequently the total energy of this gas will remain unaltered. These conditions will be fulfilled if the gas is contained in a cylinder, of which it may occupy the height  $v$ , and compressed by a weight  $P$  placed upon the (weightless) piston, which is supported at the height  $v$  by the upward pressure of the gas,  $p=P$ . So long as  $p$  and  $P$  are always equal, it does not matter whether they remain constant, or whether they alter in any manner whatever. Let  $P$  be a vessel of water allowed to run off gradually, so as to become less and less, or allowed to be filled up gradually by water flowing into it, so as to become greater and greater. Any infinitesimally small variation of  $P$  either way will necessarily be instantly followed by a corresponding change in  $p$ . If  $P$  diminishes, the gas will expand, and  $p$  will diminish also; and so, too, if  $P$  increases. But there will never be any, even instantaneous difference between  $P$  and  $p$ . There will be a swelling or a contraction of the gas; but while  $P$  remains as nothing but the equal and opposite reaction to  $p$ , no external force will be in action throughout the gas. Moreover the work done is the swelling of the gas—strictly internal work, the enlargement of the distance of the particles from one another. It must be concluded that in this experiment internal forces alone have done all the internal work, and, therefore, that there has been no variation of total energy. The total energy is known to vary as the temperature; and what is concluded of the one may be immediately transferred to the other.

From the constant equality of the forces  $P$  and  $p$  throughout the motion, it follows that the piston has risen with a uniform velocity. And we may at once convert this proposition, and say that whenever a weight is made to move upwards with a uniform velocity, the forces which act upon it are in equilibrium, and that such a raising of a weight as that *is not* an external work done at the expense of the heat or potential energy of the gas.

Let us now pass to another form of the experiment, and instead of the gradual influx or efflux of water, suppose  $P$  to be *suddenly* altered either way by the finite quantity  $\pm\pi$ . It is certain that the condensation or rarification of gas, even in a clear and unobstructed chamber, takes time. It may be made by proper contrivances to take considerable time. But I do not suppose that the same is the case with the sudden alterations of  $P$ . The change of the pressure would be felt—not perhaps to the whole extent of  $\pi$ , but partially felt throughout the gas *instantaneously*. If this be admitted, then we shall have, during the interval of time between the alteration of  $P$  and the com-



pletion of the change in the gas's density, by which a new position of equilibrium will be obtained—during this time we shall have the gas acted upon by an external force  $\pm\pi$ ; or, to keep to the language of the first part of this article, we shall have an external force doing internal work and increasing total energy if  $\pi$  is positive; and if  $\pi$  is negative, we shall have a part of our internal force, or potential energy, consumed in doing external work, generating *vis viva* in outside particles, and therefore a loss in total energy. Temperature is raised within the gas in the first place, it is lowered in the second. It is to be observed that  $\pi$  (that is,  $\pm(P-p)$  and not  $\pm P$ ) is the measure, according to my estimate, of the force which, by a given compression, will develop a given quantity of heat. In neither of these cases is the motion of the piston uniform; and we shall conclude that when a weight is raised or lowered, by changes in the density or temperature of a gas, by a motion other than uniform, the forces are not of the same name with their effects, and the total energy or the temperature of the gas is altered. But another thing must also be remarked which appears to follow from this, and which, if true, is subversive of a great deal in the modern speculations upon this subject which has hitherto been hardly called in question. It is this: the alteration of temperature caused by the action of forces of different name from their works must be of very short duration; and the whole effect, whether of heating or cooling, is neutralized as soon as the piston attains a second position of equilibrium. A good instance of this is given in Mr. Joule's experiments on the cooling of gas by explosion. In those experiments, as soon as the communication was opened between the full and the empty vessels, the full vessel was an example of the internal forces of the gas doing external work. The gas was necessarily cooled in consequence. The empty vessel was a case, as soon as there was any gas in it, of external force doing work within that gas, which was therefore warmed. But as soon as the density had become uniform throughout, which it does in an exceedingly short time, there was found to be no loss or gain of heat.

Milland, Liphook, March 14, 1870.

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XI. *Investigation of Flame-temperatures, in their Relations to Composition and Luminosity.*—First Memoir. By B. SILLIMAN and HENRY WURTZ\*.

*Calorific Powers or Effects of Gases.*

THESE subjects lie, in our belief, at the very basis of the true theory of the phenomena of luminiferous gases, and have practical bearings that can scarcely be overrated.

\* Communicated by the Authors, having been read to the American Association at Salem, August 1869.

In fact, our studies of the subject have led us in the direction of the general conclusion that, all other conditions being equal, the *temperature* in a given flame is the main factor of luminosity. This, however, may as yet be regarded merely as an hypothesis, in consequence of the imperfection of our present means of actual experimental demonstration of the temperature of flames; it is an hypothesis, nevertheless, which is in general accordance with known facts. By the spectroscope, for example, which can recognize only luminous rays, we find that the higher the temperature the greater the number of these luminous rays. The recent results of Frankland upon the development of luminosity by increased pressure in flames which are non-luminous under atmospheric pressure, are in accordance with this view, increase of temperature necessarily following increase of pressure.

Very vague views have been rife, even among chemists, with regard to the temperatures of luminiferous flames. Some have been satisfied with believing crude hypotheses, such as that the heat-power of a flame is always proportional to the *density* of the gas or vapour undergoing combustion, or that it is proportional to the *amount of oxygen consumed* by a given volume of the gas, and so on. This latter hypothesis has been one of very common acceptance. A view which is even now entertained by some skilful chemists (than which, however, nothing, as will be shown below, could be more fallacious) is, that those individual gaseous compounds which impart the highest luminosity under ordinary conditions, are also the most productive of heat.

The admirable researches of the great gas-chemist, Bunsen, of Heidelberg, placed in our possession some years ago the means of computing, at least with approximate accuracy, the heat of flames of gases of known composition. Few, however, have properly and successfully applied Bunsen's methods in practice. We consider it quite time that these methods should be introduced to the knowledge of gas-engineers in forms available to them.

Bunsen's formulæ for these computations are based upon the actual experimental determinations of the *total* amounts of heat developed by the combustion of different pure combustible gases with pure oxygen made by Favre and Silbermann, and upon Regnault's determinations of the specific heats of gaseous products of combustion.

It is not to be maintained that Favre and Silbermann's numbers are strictly correct; but they are doubtless approximate, and at least proportionally correct among themselves; at any rate they are the best data we have. Those employed here are included in the following Table. They are usually given in the

text-books for equal *weights* of the gases ; but we have reduced, them to the standard of equal *volumes* also, as more suitable to our present purpose. This reduction is made simply by multiplying the equivalents for weights by the densities as given in the third column.

TABLE I.

	Total calorific equivalents.		Densities on scale of hydrogen = 1.
	Of equal weights.	Of equal volumes.	
Hydrogen .....	34462° C.	34462° C.	1
Carbonic oxide .....	2403	33642	14
Marsh-gas .....	13063	104504	8
Olefiant gas .....	11858	166012	14

The meaning of this Table is simply that equal weights of water would be heated by the several gases to temperatures proportional to the numbers in the first column when equal *weights* of the gases are burned, and proportional to those in the second column when equal *volumes* are burned.

A cursory glance at the figures in the second column of this Table might seem to justify the notion hitherto entertained by many, of the comparatively low calorific powers of hydrogen and carbonic oxide ; and it was doubtless as a consequence of such a comparison as this that statements have been put forth and widely accepted among American gas-engineers to the effect that the weight of water heated from the freezing- to the boiling-point by one cubic foot of the four main components of illuminating gas, respectively, is as follows :—

Hydrogen . . . . .	2.22 lbs. water.
Carbonic oxide . . . . .	2.16     "
Marsh-gas . . . . .	6.17     "
Olefiant gas . . . . .	10.74    "

the figures here being obviously about in the same ratio as those in the second column of Table I. Several most grave errors, however, are here involved. To get at the true relative calorific effects of the above gases when burned in the open air in heating water below its boiling-point, deductions must be made, not only for the *specific heats* of the products of combustion of the gas, but also, more important still, for the specific heat of the *nitrogen of the air* required to burn the gas. In fact, when we consider that for each volume of oxygen required to burn a given volume of a gas about *four volumes* of nitrogen must be heated up to the temperature of the flame, it becomes easy to conceive, what

is actually the fact, that, within certain limits, the waste of heat due to this cause alone counterbalances altogether the advantage that would be supposed to result from the crowding of combustible matter into so condensed a form as in the illuminating hydrocarbons. An inevitable result of our investigations of this matter is, that the powers of the flames of pure hydrogen and pure olefant gas, even when used to the greatest advantage, to heat water below its boiling-point are almost or quite identical.

In this discussion we have occasion to use the numbers representing the specific heats of but *three gases* (the three, namely, which remain after complete combustion)—*steam, carbonic acid, and nitrogen*—as we must assume that in the hottest and most luminous zone or shell of the flame there is no oxygen in excess to be heated. These three numbers are, according to Regnault's latest determinations, for equal weights of

Steam . . . . .	0.4805
Carbonic acid . . . .	0.2163
Nitrogen . . . . .	0.2438
( <i>Liquid water</i> being . .	1.0000)

This means that the amounts of heat which would raise one pound of water and steam *to the same degree* are in the ratio of 0.4805 for the pound of steam, and 1 for the pound of water.

#### *Calculation of the Calorific Effect of Hydrogen burning in Air.*

Let us take, first, the simplest case possible, that of hydrogen with exactly the right admixture of pure oxygen to burn it, which, by Table I., developes a total heat of 34462° C. ; that is, would heat a certain weight of *liquid* water to this temperature. In order to find the actual amount of heat contained in the products of combustion, we must first take into account the fact that one pound of hydrogen burns to *nine pounds* of steam, and then obtain the ratio between the above number (34462), and the amount of heat necessary to heat nine times the weight of steam—that is, nine times the specific heat of steam. Calling the total residual heat in the produced steam *x*, we have the simple proportion :—

$$9 \times (\text{specific heat of steam} = 0.4805) : 34462 :: (\text{specific heat of water} = 1) : x,$$

or

$$x = \frac{34462^\circ}{4.3245} = 7969^\circ \text{ C.} = 14376^\circ \text{ F.};$$

\* Bunsen, in his 'Gasometry' (English edition of 1857, p. 242), gives this number as 8061° C., the difference being due to the use by him of a different number for the specific heat of steam, namely 0.475, apparently an earlier determination of Regnault. Bunsen makes here the singular



a number which, we may add, represents the *maximum* of heat capable of being imparted to *liquid water* by the flame of Hare's oxyhydrogen blowpipe.

Still we have by no means here the actual temperature of the free or open flame of Hare's blowpipe, which is generally *lower* than this figure, as we have not yet taken into account the "latent heat," or heat of vaporization, of the 9 lbs. of steam formed. The Centigrade temperature necessary to convert 1 lb. of water into steam being  $537^{\circ}$ , to get the *actual temperature* of the oxyhydrogen flame we must modify the above equation, so that

$$x = \frac{34462^{\circ} - (9 \times 537^{\circ})}{4.3245} = 6851^{\circ} \text{ C.} = 12364^{\circ} \text{ F.};$$

which is the temperature actually possible in the flame of the compound blowpipe, *were the combustion instantaneous and complete.*

When hydrogen gas burns *in air*, however, as has been before stated, another deduction of enormous amount must be made from the above figures, due to the heat required to expand the nitrogen. This is obtained simply by adding to the divisor, as above, the weight of the nitrogen of the air employed, multiplied by its specific heat. The weight of the nitrogen in air = 3.318 times the oxygen, so that the latter of the above equations becomes

$$x = \frac{34462^{\circ} - (9 \times 537^{\circ})}{4.3245 + (8 \times 3.318 \times 0.2438)} = 2744^{\circ}.5 \text{ C.} = 4972^{\circ} \text{ F.}$$

We have here a full explanation of the extraordinary rate of degradation of illuminating gas by admixture of air, which we have discussed elsewhere. The nitrogen of such air is not merely a diluent or even a mere deductive quantity, its specific heat is an actual *divisory* function in diminishing the flame-temperature.

This, then, is the actual temperature to which the flame of hydrogen gas burning in the atmosphere might attain to, supposing complete and instantaneous combustion. If it is desired to obtain instead the total calorific effectiveness, as in heating water below its boiling-point, in which case the latent heat of the steam of combustion becomes also available, the above expression is changed by simply omitting the subtrahend in the numerator:

$$x = \frac{34462^{\circ}}{4.3245 + 6.4714} = 3192^{\circ} \text{ C.} = 5778^{\circ} \text{ F.}$$

---

oversight of regarding this figure as the temperature when "the gases can freely expand, as is the case in an open flame," overlooking the correction necessary in this case for the *latent heat of steam of combustion*, as is explained in the text above.

*Calculation of the Calorific Effect of Carbonic Oxide burning in Air.*

As the product of combustion is here solely carbonic acid, no latent heat of steam enters, and the calorific effectiveness is the same under all circumstances in air. In the numerator we substitute, of course, the calorific equivalent of one volume of carbonic oxide from Table I., and in the denominator, for the specific heat of 9 lbs. of water, that of 22 lbs. of carbonic acid, being the weight of the latter formed by the combustion and combination of 14 lbs. (weight of a volume of carbonic oxide on the hydrogen-scale by third column of Table I.) of carbonic oxide with 8 lbs. of oxygen. The number for the specific heat of nitrogen is the same as before, and the equation is now

$$x = \frac{33642^{\circ}}{(22 \times 0.2163) + 6.47} = 11.23 = 2996^{\circ} \text{ C.} = 5425^{\circ} \text{ F.}$$

*Marsh-gas and Olefiant Gas.*

In these two cases we have as products of combustion both carbonic acid and water; and therefore, when the calorific effects are sought for, we have not only the latent heat of steam entering as a subtrahend into the numerator, but also into the denominator (as divisors) all three of the specific heats of steam, carbonic acid, and nitrogen.

Then, as 8 lbs. of marsh-gas consume 32 lbs. of oxygen and produce 22 lbs. of carbonic acid and 18 lbs. of steam, and as 14 lbs. of olefiant gas consume 48 lbs. of oxygen, producing 44 lbs. of carbonic acid and 18 lbs. of steam, the equations for the calorific powers of their flames in air become:—

For marsh-gas,

$$x = \frac{104504^{\circ} - (18 \times 537^{\circ})}{(18 \times .4805) + (22 \times .2163) + (32 \times 3.318 \times .2138)} = 2414^{\circ} \text{ C.} = 4386^{\circ} \text{ F.};$$

and for olefiant gas,

$$x = \frac{166012^{\circ} - (18 \times 537^{\circ})}{(18 \times .4805) + (44 \times .2163) + (48 \times 3.318 \times .2138)} = 2143^{\circ} \text{ C.} = 4970^{\circ} \text{ F.}$$

When the deduction for the latent heat of the steam of combustion is not made, the results in these two gases are considerably higher, as will be obvious from mere inspection of the formulæ.

We shall now give in tabular form all the results of our calculations of the calorific powers, when burning in the air, of the four gases we have to deal with.

TABLE II.

For equal volumes of the gases burning in air.	Calorific effects in heating liquid water.		Calorific effects above 100° C.	
	Centi-grade degrees.	Fahren-heit degrees.	Centi-grade degrees.	Fahren-heit degrees.
Hydrogen { (specific heat $11\text{O} = \cdot 4805$ )...	3192	5778	2744	4971
{ (specific heat $\text{HO} = \cdot 4750$ )...	3204	5799	2755	4991
{ (mean) .....	3198	5788	2749	4980
Carbonic oxide .....	2996	5425	2996	5425
Marsh-gas (specific heat $\text{HO} = \cdot 4805$ ) ...	2660	4820	2414	4386
Olefiant gas (specific heat $\text{HO} = \cdot 4805$ )...	2916	5481	2743	4970

*Computation of Calorific Effects of Mixed Gases.*

The above Table renders the calculation of the calorific effects of any given gaseous mixture, whose centesimal composition is known, a matter of extreme simplicity. It is only necessary to obtain the sum of the multiples of the percentage of each component gas into its calorific capacity, as given in this Table, and divide by 100\*.

To serve as examples of these modes of computation, we here cite, in tabular forms, the results of some analyses of a number of gaseous mixtures made by us during the past winter (1868-69). [These analytical results, it may be remarked, possess points of novelty and importance, both scientific and practical, which will bring them up again hereafter in other connexions. They are here placed on record.]

Table III. gives the results of two analyses of gaseous mixtures obtained by passing steam *superheated to incandescence* upwards through a mass of *anthracite coal* heated to a high degree in a clay retort of a novel construction, according to what is now known as the "Gwynne-Harris," or American Hydrocarbon Gas System. In this Table the results are calculated without carbonic acid and sulphuretted hydrogen, which, with traces of nitrogen and sometimes of oxygen, are found in the unpurified anthracite gas.

\* Professor Buusen, in the masterly discussion of the subject presented in his 'Gasometry,' not having in view the exact object we propose, has used a train of reasoning and a mode of formulation of some complexity, to follow which requires some little mathematical skill,—part of his object having been to construct a formula so general and comprehensive as to cover the direct computation, from any gaseous mixture independently, of its special calorific intensity. We have here aimed at so simplifying as to bring the whole subject within the capacity of all. Our above tabulation of the individual gaseous components, as a starting-point, seems to us to accomplish this most effectually, so far as illuminating gases are concerned.

TABLE III.—Analyses of Anthracite Hydrocarbon Gas, by Silliman and Wurtz.

	No. 1.	No. 2.	Mean.
Hydrogen .....	60.43	59.32	59.87
Carbonic oxide .....	35.44	37.14	36.29
Marsh-gas .....	4.13	3.54	3.84
	100.00	100.00	100.00

In Table IV., column 1 gives the results of the analysis of the street gas served out at this period by the New Haven Gas-Light Company, made from Westmoreland coal enriched with about 6 per cent. of Albertite; column 2 the mean of four analyses of the completed hydrocarbon gas made by us at Fair Haven during the same time, by combining gas from the same Westmoreland coal (with 10 per cent. of Albertite) with half its volume of the anthracite gas. Columns 3 and 4 are obtained from 1 and 2 by centesimal reduction, after deduction of the illuminant ingredients, being what we propose to designate as the non-illuminating *substrata* of illuminating gases.

TABLE IV.—Gas Analyses, by Silliman and Wurtz.

	1. New Haven city gas.	2. Fair Haven hydrocarbon gas.	3. Substratum of New Haven gas.	4. Substratum of Fair Haven gas.
Hydrogen .....	43.58	46.77	46.79	50.27
Carbonic oxide ...	2.14	9.56	2.31	10.27
Marsh-gas .....	47.42	36.71	50.90	39.46
Illuminants .....	6.86	6.96		
	100.00	100.00	100.00	100.00

Table V. gives the results of the computation, from our formulæ, of the calorific powers of these five gaseous mixtures, for communicating temperatures both above and below that of aqueous ebullition. We should remark that we have here been obliged to regard the volumes of *illuminant hydrocarbons* as representing olefiant gas solely—both because we have no certain data as to their real nature, and particularly because, if we actually knew or should assume the nature of the hydrocarbon vapours present, still we have no experimental calorific equivalents, as we have for olefiant gas, from which to start in such a computation. We have reason to believe, nevertheless, that the errors thus introduced are not important in amount.



TABLE V.

	Weights of water equally heated below boiling, by equal volumes.	Weights of water equally heated above boiling, by equal volumes.	First column reduced to New Haven gas = 100.	Second column reduced to New Haven gas = 100.
Anthracite gas .....	3100	2823	104.2	109.2
Substratum of the New Haven street gas.....	2917	2581	98.1	99.6
Substratum of the Fair Haven hydrocarbon gas .....	2962	2640	99.6	102.0
New Haven gas, with the illuminants as- sumed = olefiant ...	2974	2592	100.0	100.0
Fair Haven gas, with the illuminants as- sumed = olefiant ...	2959	2647	99.5	102.1

*Conclusions.*

Some of the practical conclusions to which we are of necessity compelled by the results of the above investigations are somewhat remarkable, so that we feel diffident regarding them. It is, however, always safe to follow the leading of Truth, however astray she may conduct us from our preconceived notions.

From Table II. it is apparent—

1. That, of all known gases, the highest calorific effects, under ordinary atmospheric conditions, are obtainable from *carbonic oxide*, whose calorific value, above  $100^{\circ}$  C., is about  $3000^{\circ}$  C.

2. That in absolute calorific value, below  $100^{\circ}$  C., in the atmospheric medium, *hydrogen* surpasses its volume of any other gas, giving a temperature of about  $3200^{\circ}$  C.

3. That for all modes of application (that is, for producing both high and low temperatures) the total maximum calorific effectiveness of carbonic oxide is a *constant quantity*\*.

4. Compound condensed submultiple volumes of hydrogen, like that in marsh-gas, have much *less* total calorific value in air than their volume of free hydrogen.

5. Condensed compound submultiple volumes of gaseous carbon, like that in olefiant gas, have no greater total calorific value in air below  $100^{\circ}$  C. than their own volume of carbon-gas in the form of carbonic oxide, while above  $100^{\circ}$  C. their value is even considerably less.

\* Metallurgists especially will appreciate the suggestive import of the truths presented under the first and third heads, here enunciated, as we think, for the first time. It is to be noted that all the above effects belong to the *maximum* kind, and, of course, reach their development only under the most favourable conditions in each case respectively.

XLI. *Solar Radiation in connexion with Cloud and Vapour.*

By J. PARK HARRISON, Esq.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

THE results of some observations of solar radiation as affected by cloud, which were communicated to the Royal Society at their last Summer Meeting in 1869, having been thought of sufficient interest to be reprinted in "The Proceedings of Societies" in your Journal for January, will you permit me to explain that they were intended to be followed by details of experiments with smaller screens, on perfectly cloudless days? It was hoped they might have been completed in the autumn of last year, and some progress was made in July; but the close and continuous examination of the state of the sky up to the very edge of the sun's disk, which was requisite in order to detect the existence of any light form of cloud, proved so trying to the sight, that a complete cessation for some time of all work of this description was found to be absolutely necessary.

From a few observations made in October and January, however, I believe I may state that the accession of heat from glare in our atmosphere  $5^{\circ}$  to  $8^{\circ}$  round the sun, compared with the heat received from direct solar radiation, is very nearly in the ratio 1 : 3; but this must be taken subject to correction when the sun is in higher altitudes. The experiments were made in these months under very equal conditions, with the solar thermometer raised several inches above short grass and on perfectly calm days\*.

The increase of insolation observed when light *clouds* approach the sun was shown in the paper above referred to to be very much greater; whilst diffused light and heat, from white cloud and vapour beyond  $20^{\circ}$  from the sun, causes no perceptible difference in the readings of an exposed black-bulb thermometer.

When the relation of insolation to humidity which was brought before the Royal Society by H. v. Schlagintweit† (without his being aware that Mr. Forbes had noticed the same thing some years previously at Edinburgh) was found to be supported by observations of the actinometer and solar thermometer at Greenwich, I too hastily concluded that effects which appeared to be undoubtedly connected with cloud and vapour were due wholly to counterradiation of heat received from the bulb of the solar thermometer and the ground generally, as in the case of a

\* The disks used as screens were placed 6 inches from the bulb of a solar thermometer  $\frac{1}{8}$  inch in diameter. A disk  $\frac{1}{16}$  inch diameter, at the above distance, was assumed to be amply sufficient to screen the sun.

† Proceedings of the Royal Society, 1865, vol. xiv. p. 111.

radiation-thermometer at night\*. With the data, however, now afforded by the experiments with screens, and the eye-observations of cloud with which they were associated, it cannot, I think, be doubted that the peculiar stinging sensation experienced by H. v. Schlagintweit in India (apart from the high readings of insolation which he observed in regions of great relative humidity) must have been owing to the diversion of rays of small obliquity by light cloud high up in the atmosphere; they consequently reached the bulb of his solar thermometer almost in a direct course and with little loss of heating-power.

I am not aware whether the fact that so great an accession of heat is attributable to the near approach of light cloud to the sun has previously been observed. Certainly, as regards my own experience, the not uncommon sensation of a sudden increase of solar heat under a partially clouded sky was always assumed to be due, not to light cloud *approaching* the sun, but to its passing off from it—or, if the sky appeared, on a superficial view, to be clear and cloudless, to some possible outburst of heat in the sun itself.

The question whether the direct rays of the sun are more intense in proportion as they pass through a limited extent of atmosphere is scarcely an open one. But it will be seen that the admission of loss of heat from absorption in no wise militates with the fact that a high degree of insolation may be, and sometimes is obtained, even at comparatively low altitudes of the sun, under atmospheric conditions which favour counter-radiation, or, in the case of cloud near the sun, by complete reflection of the solar rays.

I am,

Yours faithfully,

Ewhurst, March 1, 1870.

J. PARK HARRISON.

### XLIII. *Notices respecting New Books.*

*Our Domestic Fireplaces. A New Edition. By* FREDERICK EDWARDS, Jun. London: Longman and Co., 1870. (Pp. 168, with many illustrations.)

**T**HIS work has been "entirely rewritten and enlarged, the additions completing the author's contributions on the domestic use of fuel and on ventilation." It consists of a short historical account of open fireplaces, and of the various recent projects for improving them. The author's principal suggestions are made under the following heads:—(1) The best form of grate should be used—that, namely, which presents the largest amount of radiating or reflect-

\* Proceedings of the Royal Society, February 1867; and Phil. Mag. vol. xxxiii. p. 391.

ing surface to the room. (2) The most suitable materials must be used for the construction of the grate. The back and sides should be made of fire-bricks, fire-lumps, or fire-stones, not on any account of cast iron. For ordinary use, Welsh are better than Stourbridge fire-bricks; for though they do not stand intense heat, they are less liable to be broken by a blow of a poker. (3) The best form of fire-bars must be used; our author, however, does not make it plain which form is the best, straight bars having one advantage, curved bars another. He tells us that for a room 10 feet each way (containing, therefore, 1000 cubic feet) 12-inch bars are long enough; and there should be two additional inches for every additional thousand feet of cubic contents. (4) The fire should be kept in a basket of fire-brick, and the supply of air from below should be checked. (5) The escape of the warm air of the room into the chimney should be checked. This is to be done by improving the form of the chimney, and by placing in the chimney a door that can be opened or shut by hand, instead of the ordinary register-valve, which in practice is rarely used. By these means no more air is allowed to pass up the chimney than is needed for carrying off the smoke. (6) Air for combustion should be supplied from a source near the fireplace instead of from the doors and windows. This may be done by bringing an air-channel under the floor from the outside wall to the hearthstone, the mouth of the channel being capable of being closed by a sliding valve. By this means the fire is fed by cold air from without instead of by the warm air of the room. It may be added that the author urges that windows should be double glazed, and that the heat which escapes by the chimneys should be utilized.

Any one engaged in building a house or refitting a house with stoves, will find the book well worth looking into, both for the many useful hints which it contains, and for the diagrams with which it is illustrated. In this view the book is the more to be relied on, as we believe that Mr. Edwards is practically engaged in stove-making, and, we may add, he has clearly paid great attention to the principles of his art. He states in his preface that "the matter of the" volume "has been due to his own investigations;" but we suppose he would allow that many of his suggestions have been made by others as well as by himself.

Regarded from a literary point of view, the book contains several passages which are somewhat curious specimens of style and sentiment. That on p. 16 is too long to quote, where the author pays his tribute to the memory of the bellows, the warming pan, the folding screen, and the eight-day clock. Another, on p. 35, which describes the extinction of hobs, runs as follows:—"Housewives regretted the departure of the hob, but became reconciled to the brass footman or hanging trivet. The time-honoured hob, however, struggled to hold its own. It came out ornamental and put lower down, and was cleverly recessed in corners; but nothing could save its gradual departure, and it is now becoming rapidly unknown."



*Lecture-notes on Physics.* By ALFRED M. MAYER, Ph.D., Professor of Physics in the Lehigh University, Bethlehem, Pa.—Part I. Philadelphia: from the Journal of the Franklin Institute, 1868 (pp. 112).

This is scarcely to be called a book, it is much more nearly a synopsis of the subject, with disquisitions inserted on the points of chief interest, as well as extracts from other writers. The subjects discussed in this first part are the following:—Introduction to the Inductive method, Instruments for precise measurements, Methods of Precision (*e. g.* the method of least squares), Methods of determining the laws of phenomena, the General Properties of Matter, and Capillary Attraction. The author gives a very good *résumé* of the leading facts under each of these heads, and, more than that, gives many references that may be useful to those who wish to go further into the subject than he is able to take them. The following short extract will perhaps convey a better notion of the form of the book than an elaborate description. Under the head of instruments for measuring lengths, the author gives an account of the micrometer-screw. This he does as follows:—

“*Micrometer-screw* (from Greek *μικρος* small, and *μετρον* a measure) consists of a screw with a large circular head, whose circumference is divided into a certain number of equal parts. Suppose the screw has fifty threads to the inch, and that its circular plate, which rotates with it, is divided into two hundred parts; then if the screw is turned a whole revolution, it will advance, in the block in which it turns,  $\frac{1}{50}$ th of an inch; but if the head is revolved only through  $\frac{1}{200}$ th of a revolution, the screw will advance the  $\frac{1}{10,000}$ th of an inch. With a good micrometer-screw we can measure accurately the  $\frac{1}{1000}$ th of a millimetre, or about the  $\frac{4}{100,000}$ th of an inch.

“(Large model exhibited, also the instrument itself in various forms.)” (P. 16.)

We have only to add that, allowance being made for the imperfections necessarily arising from the *form* of the book, it seems to us well done, and to be the work of a man who has an accurate and extensive acquaintance with his subject.

*Theory of Existence.*—Part I. devoted to the enunciation of the laws which determine the motions that result from the collision of ponderable bodies. By ELIAS DEXTER. New York: Edward Dexter, 564 Broadway, 1869. Pp. 155.

The object of this first part of the above work is to dispel what the author regards as prevalent *errors* about the laws of motion and other fundamental points of mechanical philosophy. He aspires to take the same place with respect to Newton and Laplace that Galileo took with respect to the Aristotelians of his day. How the author may deal with the General Theory of Existence, when he gets to it, we do not know; but his power of dealing with elementary dyna-

mical questions may be judged of by the following indication. His definition of velocity runs thus:—"The velocity of a body is determined by multiplying the weight of the body by the space passed over in a given time" (p. 1). Now not only is there a want of precision in the statement (a serious fault in such a matter), but what is determined in the way stated is not what is generally termed velocity. The author might possibly object that this is a question of words; yet it is a strange use of language which implies that the velocity of a horse is many times greater than that of his rider; and, moreover, a few pages after, he expresses the velocities of bodies in the usual way of so many feet per second (p. 7, p. 26, &c.).

A large portion of the volume is taken up by a statement of "the laws which determine the results of collision;" and these statements are applied to what may in courtesy be called a *criticism* of Newton's laws of motion. As a specimen of our author's laws, we will quote the first in the book (p. 3): A and B, it must be premised, are two spherical bodies, the weight of A being less than, or equal to, that of B. The former moving with a given velocity along the line of centres impinges on the latter at rest, then:—"Law I. A will be reflected back from the point of collision in the same line that it moved in before collision. Both the velocity and momentum of A, as compared with the velocity and momentum of A before collision, will be proportional to the difference of the respective weights of A and B."

A good many points are suggested by this "law:" *e. g.* *First*, it is assumed as an ultimate fact, without any reason assigned. *Secondly*, it is not *stated*, though incidentally it is *assumed*, that the colliding bodies are perfectly elastic (case 100, p. 10). *Thirdly*, the "law," as it stands, means that

constant  $\times$  A's velocity of rebound =  $(B - A) \times$  A's velocity of impact.

As the constant must be finite, it follows that if a boy drops a marble on a pavement it rebounds with an indefinitely great velocity. *Fourthly*, the words of the "law," however, do not convey the author's meaning; that, as appears from his treatment of particular cases (pp. 4, 5), seems to be,

$B \times$  A's velocity of rebound =  $(B - A) \times$  A's velocity of impact.

This, however, does not much mend the matter, for it is not the true law. It is not necessary to continue our remarks; if it were, they could be continued indefinitely. What we have said will show the reader that to buy the book is waste of money, to read it waste of time.

XLIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 232.]

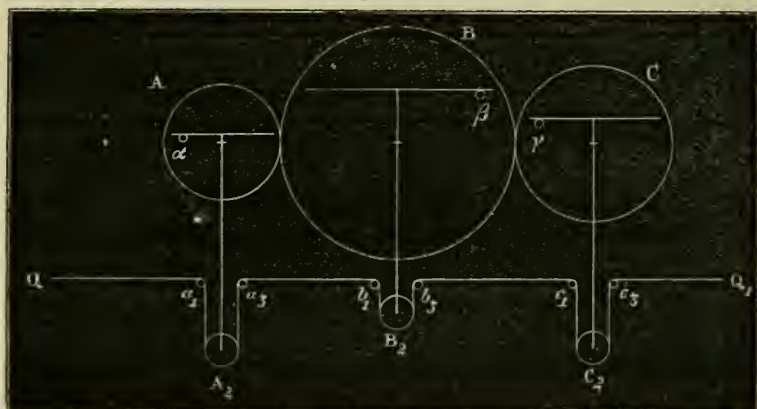
June 17, 1869.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

“On the Mechanical Description of Curves.” By W. H. L. Russell, F.R.S.

Let A, B, C be three wheels rolling in one another (fig. 1); they may of course be supposed to describe simultaneously the angles  $m\theta$ ,  $n\theta$ ,  $r\theta$ , when  $m$ ,  $n$ , and  $r$  are constant.

Fig. 1.

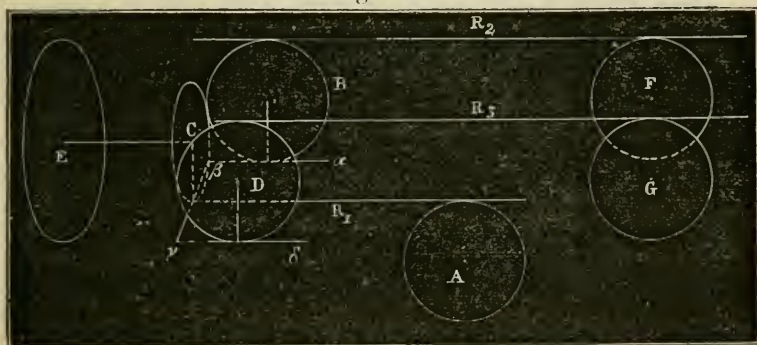


Let  $\alpha, \beta, \gamma$  be three nuts situated on A, B, C respectively, at distances  $a, b, c$  from their centres. Then if these nuts work in horizontal bars (as exemplified in many sewing-machines), the bars will descend vertically through the spaces  $a \sin m\theta$ ,  $b \sin n\theta$ ,  $c \sin r\theta$  respectively. We may combine all these vertical motions together; for if vertical rods be attached to the horizontal bars, and a cord fixed at Q pass over the pulleys  $a_1, A_2, a_3, b_1, B_2, b_3, c_1, C_2, c_3$ , as shown in the figure, the other extremity  $Q_1$  will describe the space  $a \sin m\theta + b \sin n\theta + c \sin r\theta$ . By this contrivance we are able to combine any number of vertical descents, so that it is readily seen that  $a \sin (m\theta + \alpha) + b \sin (n\theta + \beta) + \&c.$  may be described mechanically. A machine on the same principle as this had been previously invented by Mr. Bashforth.

I soon perceived that in order to describe the general equation of the  $r$ th order by continued motion, it was necessary to make a wheel revolve through an angle equal to the sum and difference of the angles described in the same time by two given wheels; to effect this I invented the apparatus shown in fig. 2.

In fig. 2 let A be a vertical wheel working truly in a horizontal rack  $R_1$ , which propels the horizontal frame  $\alpha, \beta, \gamma, \delta$ . On this

Fig. 2.



frame stand the wheels B and D parallel to the plane of the paper. The wheel C, supposed perpendicular to the plane of the paper, works by teeth in the wheels B and D, and the four wheels A, B, C, D are precisely equal.

To the centre of C is attached a square axis, which passes through the centre of the wheel E, so that the wheel E in revolving may, without changing its plane, communicate motion to C as the frame moves forward. Two horizontal racks,  $R_2, R_3$ , parallel to the plane of the paper, are urged by the wheels B and D; and these, again, work in the fixed wheels F and G, equal to A, B, C, D in all respects. Then, if the wheel A describe in a given time the angle  $\theta$ , and the wheel E in the same time the angle  $\phi$ , the wheels F and G will revolve respectively in the same time through the angles  $\theta + \phi$  and  $\theta - \phi$ .

We shall call the wheel A an abscissa wheel, the wheel E an ordinate wheel, for reasons which will appear directly, also F an addition wheel, and G a subtraction wheel.

Let  $x = a \sin \theta$ ,  $y = a \sin \phi$ , then the general equation of the  $r$ th order may be written

$$\alpha \sin (m\theta + n\phi) + \alpha' \sin (m'\theta - n'\phi) + \alpha'' \sin (m''\theta + n''\phi) + \dots = a \sin \theta.$$

Let a number of machines like the foregoing be placed side by side with their ordinate wheels rolling in one another, and their abscissa wheels duly connected. Let one abscissa wheel describe an angle  $m\theta$ , and the corresponding ordinate wheel the angle  $n\phi$ , then a nut placed on the corresponding addition wheel, at a distance  $\alpha$  from its centre, will cause a horizontal bar to descend vertically through a space  $\alpha \sin (m\theta + n\phi)$ . In the same way a nut properly placed on the subtraction wheel will cause a horizontal bar to descend vertically through a space  $\alpha' \sin (m'\theta - n'\phi)$ . By means of the adjacent machines we may in like manner cause bars to descend through the vertical spaces,  $\alpha'' \sin (m''\theta + n''\phi)$ ,  $\alpha''' \sin (m'''\theta - n'''\phi)$ , &c. Now let motion be communicated to the ordinate wheels, and let all the



vertical motions due to the addition and subtraction wheels be combined together and made to act vertically upon a nut in one of the abscissa wheels ; then the angles  $\theta$ ,  $\phi$  will satisfy the equation

$$\alpha \sin (m\theta + n\phi) + \alpha' \sin (m\theta - n\phi) + \alpha'' \sin (m'\theta + n'\phi) \dots = \alpha \sin \theta,$$

which is the general equation of the  $r$ th order.

Therefore two bars moved respectively horizontally and vertically by nuts in the wheels describing the angles  $\theta$  and  $\phi$  will trace by their intersection the required curve.

December 16.—Lieut.-General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communications were read :—

“On the Thermodynamic Theory of Waves of Finite Longitudinal Disturbance.” By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edinb.

The object of the present investigation is to determine the relations which must exist between the laws of the elasticity and heat of any substance, gaseous, liquid, or solid, and those of the wave-like propagation of a finite longitudinal disturbance in that substance—in other words, of a disturbance consisting in displacements of particles along the direction of propagation, the velocity of displacement of the particles being so great that it is not to be neglected in comparison with the velocity of propagation. In particular, the investigation aims at ascertaining:—in the first place, what conditions as to the transfer of heat from particle to particle must be fulfilled in order that a finite longitudinal disturbance may be propagated along a prismatic or cylindrical mass without loss of energy or change of type—the word type being used to denote the relation between the extent of disturbance at a given instant of a set of particles and their respective undisturbed positions ; and, secondly, according to what law the type of a wave of finite longitudinal disturbance must change when the substance through which it is propagated has, under the circumstances of the disturbance, no appreciable power of transferring heat from particle to particle, being in the condition which, in the language of thermodynamics, is called *adiabatic*. The disturbed matter in these inquiries may be conceived to be contained in a straight tube of uniform cross section and indefinite length.

The investigation is facilitated by the use of a quantity which the author calls the *Mass-velocity* or *Somatic Velocity*—that is to say, the mass of matter through which a disturbance is propagated in a unit of time while advancing along a prism of the sectional area unity—also by expressing the relative positions of a series of transverse planes that travel along with a wave by means of the masses of matter contained between them, instead of by their distances apart.

Let such a transverse advancing plane coincide with that part of a wave of longitudinal disturbance at which the pressure  $P$  and bulkiness  $S^*$  are equal to those corresponding to the undisturbed condi-

\* The word *bulkiness* is used to denote the reciprocal of the density.

tion; it is shown that the value of the square of the mass-velocity is

$$m^2 = - \frac{dP}{dS} \dots \dots \dots (A)$$

The linear velocity of advance of the wave is obviously  $mS$ .

Let a second transverse plane advance along with the wave in such a manner that an invariable mass of matter is contained between it and the first advancing plane. The condition of *permanence of type of disturbance* is, that the distance between those planes shall be invariable. Let  $\frac{dx}{dt}$  be the rate at which that distance varies, being positive when the second plane gains on the first plane; it is shown that this quantity has the following value—

$$\frac{dx}{dt} = \frac{p-P}{m} - m(S-s), \dots \dots \dots (B)$$

in which  $p$  and  $s$  respectively are the pressure and bulkiness at the second plane. Hence the condition of permanence of type is expressed symbolically as follows:—

$$\frac{p-P}{S-s} = - \frac{dp}{ds} = - \frac{dP}{dS} = m^2 \text{ (a constant)}. \dots \dots (C)$$

This relation between pressure and bulkiness is not fulfilled by any known substance when either in an absolutely non-conducting state (called, in the language of thermodynamics, the *adiabatic* state) or in a state of uniform temperature. In order that it may be fulfilled, transfer of heat must go on between the particles affected by the wave-motion, in a certain manner depending on the *thermodynamic function*. The value of the thermodynamic function is

$$\phi = Jc \text{ hyp log } \tau + \chi(\tau) + \frac{dU}{d\tau}, \dots \dots \dots (D)$$

in which  $J$  is the dynamical equivalent of a unit of heat,  $c$  the real specific heat of the substance,  $\tau$  the absolute temperature,  $\chi(\tau)$  a function of the absolute temperature, which is  $=0$  for all temperatures at which the substance is capable of approximating indefinitely to the perfectly gaseous state, and  $U$  the work which the elastic forces in unity of mass of the substance are capable of doing at the constant temperature  $\tau$ . The thermodynamic condition to be fulfilled by a wave of permanent type is expressed by

$$\oint \tau d\phi = 0. \dots \dots \dots (E)$$

In applying this equation to particular cases,  $\phi$  and  $\tau$  are to be expressed in terms of  $p$  and  $s$ .

It is shown to be probable that the only longitudinal disturbance which can be propagated with absolute permanence of type is a sudden disturbance, and that the consequence of the non-fulfilment of the condition of permanence of type is a tendency for every wave of

gradual longitudinal disturbance to convert itself by degrees into a wave of sudden disturbance. But although suddenness of disturbance may be approximated to, it cannot be absolutely and permanently realized; whence it follows that the propagation of waves of longitudinal disturbance of absolutely permanent type for an indefinite distance is impossible; and this may be the cause of the absence of longitudinal vibrations from rays of light.

The laws of the advance of *adiabatic waves* are investigated—that is, waves of longitudinal disturbance in which there is no transfer of heat, and in which consequently  $d\phi=0$ ; and it is shown, by the aid of the equation marked (B) in this abstract, that the compressed parts of those waves tend to gain upon and at last overtake the rarefied parts, just as the crests of rolling waves in shallow water gain upon and at last break into the troughs, the consequence being a gradual conversion of the adiabatic waves into waves of sudden disturbance, followed by a mutual interference of the compressed with the rarefied parts, which leads to the energy of the waves being spent in molecular agitation.

It is also shown that the extreme values of the pressure and of the bulkiness are constant during the change of type, and consequently that the respective velocities with which the plane of greatest compression gains upon and the plane of greatest rarefaction falls behind the plane of undisturbed density are uniform.

The values of the linear velocity of advance,  $mS$ , found for various modes of finite disturbance, all approximate, when the disturbance becomes indefinitely small, to the well-known value of the velocity

of sound, viz.  $\sqrt{\left\{ \frac{dP}{d \cdot \bar{S}} \right\}}$ , the relation between  $P$  and  $S$  being de-

termined by the condition  $d\phi=0$ .

### Supplement.

In this supplement the author of the paper refers to the previous investigations on waves of finite longitudinal disturbance by the following authors:—

Poisson, 'Journal de l'Ecole Polytechnique,' vol. vii. cahier 14, p. 319.

Stokes, Philosophical Magazine, Nov. 1848, S. 3. vol. xxxiii. p. 349.

Airy, Philosophical Magazine, June 1849, S. 3. vol. xxxiv. p. 401.

Earnshaw, Philosophical Transactions, 1860, p. 133.

He points out to what extent the results arrived at in his own paper are identical with those of the above-mentioned previous researches; and he claims the following results as new:—The conditions as to transfer and transformation of heat which must be fulfilled in order that permanence of type may be realized, exactly or approximately, in a wave of finite longitudinal disturbance in any elastic medium; the types of wave which enable such conditions to be fulfilled with a given law of the conduction of heat; the velocity of advance of such waves; and some special results as to the rate of change of type in

adiabatic waves. He also claims as new the method of investigation by the aid of *mass-velocity* and *mass-coordinates*, which he alleges to possess great advantages in point of simplicity.

“On Approach caused by Vibration.” By Frederick Guthrie.

The author observes that when a vibrating tuning-fork is held near to a piece of cardboard, the latter has a tendency to approach the fork. Starting from this experiment, a series of experiments are described having for their object the determination of the cause and conditions of the fundamental observed fact.

It is shown that no sensible permanent air-currents, having their source at the fork's surface, are established—and hence that the approach of the card to the fork is not due to the expansion of such currents, as in M. Clément's experiment.

The modifications are examined which Mr. Faraday's surface-whirlwinds on a vibrating tuning-fork undergo when the fork vibrates in the neighbourhood of a sensibly rigid plane.

It is shown that a delicately suspended card approaches the fork when either of the three essential faces of the fork is presented to the card, and that the approach takes place from distances far exceeding the range of Mr. Faraday's air-current. That the action between the card and fork is mutual is shown by suspending the latter. Also one vibrating fork tends to approach another in whatever sense their planes of vibration may be towards one another.

The mean tension of the air surrounding a vibrating fork is examined by enclosing one limb of the fork in a glass tube. It appears that the vibrating fork displaces air.

The question whether the equilibrium between two equal and opposite forces acting on a body is disturbed by submitting one of the forces to successive, rapid, equal and opposite alterations in quantity, is answered in the negative by an experiment which shows that the equilibrium of a Cartesian diver is not disturbed by submitting the water in which it floats to vibration.

Various modifications are introduced into the nature of the surface which receives the vibrations, such as making it a narrow cylinder with one end closed, making it of cotton-wool, &c. It is found that in all cases the suspended body approaches the vibrating one.

The author concludes that the effect of apparent attraction is due to atmospheric pressure, and that this pressure is due to undulatory dispersion. It is suggested that the dispersion of the vibrations which constitute radiant heat may cause bodies to approach, being pushed not pulled.

February 24, 1870.—Lieut.-General Sir Edward Sabine, K.C.B.,  
President, in the Chair.

The following communication was read:—

“On the successive Action of Sodium and Iodide of Ethyle upon Acetic Ether.” By E. Frankland, F.R.S., and B. F. Duppá, Esq., F.R.S.

In a paper by Mr. J. Alfred Wanklyn, bearing the above title,



and published in the Proceedings of the Royal Society, vol. xviii. p. 91, the author refers to our memoir on the same subject printed in the Philosophical Transactions for 1866, vol. clvi. p. 37, and expresses his opinion that our interpretation of the nature of the reaction must be erroneous because it involves the disengagement of hydrogen. This opinion is founded upon certain experiments which Mr. Wanklyn has himself made, and which are described in the number of Liebig's 'Annalen' for January 1869, and in the Chemical Society's Journal, vol. ii. p. 371.

In reference to this opinion we have to remark, first, that it is founded upon experiments which differ essentially from our own, and, second, that even the results obtained in those experiments by the author do not warrant the conclusion, at variance with ours, which he has drawn from them, viz. that the evolution of hydrogen in this reaction is inadmissible.

The reaction, the theoretical explanation of which Mr. Wanklyn seeks to controvert, is described in the Philosophical Transactions, vol. clvi. p. 38, as follows :—"When acetic ether is placed in contact with sodium it becomes hot, and a considerable quantity of gas is evolved, which, after being passed first through alcohol and then through water, burns with a non-luminous flame, and the products of combustion do not produce the slightest turbidity on agitation with baryta-water. In fact the gas is pure hydrogen. When the action is complete, the liquid solidifies on cooling to a mass resembling yellow beeswax. By putting the sodium into the acetic ether as just described, it is difficult to conduct the operation to completion, owing to the liquid gradually assuming such a thick and pasty condition as to prevent the further action of the sodium." Owing to the difficulty of carrying the reaction far enough in this way, we frequently employed a modification of this process, which is minutely described in the same memoir. The modification consisted in placing the sodium in a separate vessel and causing the acetic ether to distil continuously over it; thus the portions of acetic ether still unacted upon were brought, again and again, into contact with the sodium, whilst the non-volatile product of the operation was retained in a lower vessel. As we acted upon several pounds of acetic ether at once, the operation frequently lasted several days, and *during the whole time torrents of hydrogen were evolved*. The temperature of the liquid in the distillation vessel was allowed to rise to 130° C., and the amount of sodium consumed was not much less than one atom for each molecule of acetic ether employed.

We have made several attempts to determine quantitatively the volume of hydrogen given off from a known weight of sodium, and also from a known weight of acetic ether; but in neither operation could we obtain a trustworthy result :—in the first case because the sodium, which fuses during the reaction, breaks up into a vast number of very minute globules, the final disappearance of which in the highly coloured and pasty product it is impossible to verify; in the second case because the thickening of the liquid prevents the reaction being pushed far enough to decompose the whole of the

acetic ether employed. In a quantitative experiment, in which 4·857 grammes of acetic ether were acted upon by sodium in slight excess, 344·79 cub. centims. of hydrogen at 0° C. and 760 millims. pressure were obtained. If one molecule of acetic ether had lost one atom of hydrogen, 615·9 cub. centims. of gas ought to have been collected. It was evident, however, that a large proportion of acetic ether still remained unattacked at the close of the experiment.

Such, then, was our mode of operating: the hydrogen evolved was allowed freely to escape, the whole process was conducted at the ordinary atmospheric pressure, and the temperature varied from the boiling-point of acetic ether to 130° C. Moreover the acetic ether used was prepared with the greatest care, so as to ensure the absence of alcohol and water. By our method of preparation, described in the memoir already cited, no traces of the former could be detected even in the crude ether; nevertheless it was first placed for several days over fragments of fused calcic chloride, which apparently remained perfectly dry and unaffected; it was then in some cases boiled for ten days or a fortnight upon many pounds of sodium-amalgam, which we find to be entirely without action upon pure acetic ether, whilst it rapidly attacks and removes alcohol, if the latter be added even in very small proportion to the acetic ether. When acetic ether, so treated and then distilled from the sodium-amalgam, was brought into contact with the sodium, an abundant evolution of hydrogen immediately commenced, and continued during the entire treatment, which, as already remarked, frequently lasted several days. The general impression, however, produced upon us by the whole of our operations was, that the evolution of hydrogen was not quite so great as that theoretically required by the reactions which we believe to take place; nevertheless it was obvious that no equations from which free hydrogen was excluded could possibly express correctly the chemical changes effected in this action. Certain experiments were undertaken to trace the missing hydrogen; but as they have not hitherto been completed, we will not further allude to them here.

We now turn to Mr. Wanklyn's mode of experimenting. This is not stated in his communication to the Royal Society, but is given in the *Journal of the Chemical Society*, vol. xvii. p. 371, and in the *Ann. Chem. u. Pharm.* for January 1869, as follows:—

*Exp. 1.* "I sealed up a quantity of sodium with acetate of ethyl, which had been very carefully deprived of alcohol and water, and weighed the tube containing these materials. I then heated the tube to 130° C. for some time, until the contents had changed from liquid to solid. After opening the tube and allowing any gas that might have formed to escape, I weighed it again. *The loss amounted to 0·5 in 100 parts of acetic ether.*"

*Exp. 2.* "5 cub. centims. of good acetate of ethyl and 0·3 grm. of sodium were sealed up in a small glass tube and heated in a water-bath to 100° C. until all the sodium had disappeared. The tube was then opened under water; *the evolved gas measured 25 cub. centims. at ordinary temperature, but at 0° C. and 760 millims. pressure and dry it measured 23 cub. centims.* If the volume of hydro-

gen be calculated which is equivalent to 0.3 grm. sodium, it will be found to be 140 cub. centims."

*Exp. 3.* "Another specimen of acetic ether, which was prepared with greater care, evolved no gas by the action of potassium or sodium."

It is thus evident that, whilst we allowed all evolved gas freely to escape, Mr. Wanklyn operated in sealed tubes under great pressure—an alteration in the conditions of the experiment which might well lead to a modification of the result. Mons. L. Cailletet has recently shown that the evolution of hydrogen from zinc and hydrochloric acid is gradually diminished and finally stopped under increasing pressure; and the same chemist also finds that the evolution of hydrogen from sodium-amalgam and water is diminished and finally stopped in a sealed tube. It follows from these experiments that pressure retards or even interrupts a reaction in which a permanent gas is evolved, whilst it is known to exercise little or no influence upon other chemical changes in which no evolution of gas takes place. This influence of pressure upon certain kinds of chemical action affords an explanation of the difference between the results of Mr. Wanklyn's experiments and our own, as regards the evolution of hydrogen during the action of sodium upon acetic ether. We can confirm his observation that sodium dissolves in valeric ether, under ordinary atmospheric pressure, without the evolution of any gas. A reaction, whatever its nature may be, which thus proceeds readily with ethylic valerate can scarcely be impossible with its homologue, acetic ether, and it is probable that this reaction goes on side by side with those which we have described in our memoir; but when the pressure is moderate those changes chiefly take place which involve the disengagement of hydrogen, whilst under the great pressure arising in sealed tubes these changes are more or less suppressed, and the reaction observed by Mr. Wanklyn comes into prominence.

Lastly, Mr. Wanklyn's own experiments scarcely justify his unqualified opinion that "equations which assume evolution of hydrogen in these reactions are inadmissible." In two out of three of his experiments, hydrogen in considerable quantity was evolved; and although in experiment No. 2, given above, he attributes the hydrogen to the presence of alcohol, yet in experiment No. 1 its origin cannot be so explained, as he states expressly that the acetic ether employed "had been very carefully deprived of alcohol and water;" yet the proportion of hydrogen evolved in this case was much larger than in experiment No. 2.

We reserve our observations upon Mr. Wanklyn's views regarding the changes which take place when sodium acts upon acetic, butyric, and valeric ethers, until the publication of the experimental data upon which those views are founded.

## GEOLOGICAL SOCIETY.

[Continued from vol. xxxviii. p. 468.]

May 12th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President in the Chair.

The following communications were read:—

1. “On some of the results arising from the bedding, joints, and spheroidal structure of the Granite on the Eastern side of Dartmoor, Devonshire.” By G. Wareing Ormerod, Esq., M.A., F.G.S.

After noticing the apparent bedding of the granite, the author stated that in various places, as at Scarrey and Belstone Tors on the north, Kestor and Middleton near Chagford, Blackingstone near Moreton, and Houndtor near Ilsington, the beds dipped, and the contour of the country was there caused by these curves. The joints ran in directions mostly from N. to S. and E. to W. The N. and S. joints were generally nearly perpendicular; the E. and W. often inclined to the N. or S. Descriptions of the chief forms of joints and peculiarities connected with them were given; and it was stated that decay acted along these lines, and that to them the forms of the Tors may be traced. Examples were given showing the effect of joints in the large massive tor and in the insulated rock pillar. A spheroidal structure was shown to exist in the coarse granite south of the Teign, and the possible connexion of the Rock Basins with it was noticed. Various localities were mentioned where the structure was to be seen in cuttings, and in masses like boulders but actually *in situ*. To this cause the form of rounded insulated rocks was attributed. In conclusion, the author stated that these three causes had acted frequently together, and that he considered that to them the origin of the Logans, or Rocking-stones, was often to be attributed. The Logans at Belstone and Thornworthy he attributed to the action of these three causes; and that at Rippon Tor to bedding and joints. The Drewsteignton Logan he considered a transported block, and the “Nuterackers” at Lustleigh a rock which had rolled down from the high ground above.

2. “Notes on apparent Lithodamous perforations on the Hills of North-west Lancashire.” By D. Mackintosh, Esq., F.G.S.

The author described certain perforations discovered by him in the limestone rocks near Morecambe Bay at altitudes varying from 200 or 300 to 667 feet above the sea. He stated that the course of these perforations seemed to be irrespective of any differences in the hardness of the rock, and hence, and from the regularity and smoothness of the cavities, he argued that they could not be the result of the chemical and mechanical action of the atmospheric moisture. The perforations were said to occur chiefly in groups, often ramifying from a common entrance; and where the actual entrance is preserved, this is narrower than the more deeply seated portions. The author maintained that the hollows described by him had been *ground out*



of the rock; and he expressed his belief that they were made by some animal when "the land was submerged to the extent indicated by the altitudes at which they occur." From their position he supposed their formation to have taken place during, or immediately before the Glacial epoch.

Mr. J. GWYN JEFFREYS remarked that Mr. A. Tylor had already called attention to the same subject three years ago. He could not agree in regarding the markings as lithodomous borings. The borings of *Savicava* and *Gastrochena* were not parallel, but enlarged towards the base into a pear-shaped form. They were also comparatively straight, and not curved or bifurcated as in the limestones exhibited. The range in height was also against their being the work of marine mollusks. He thought the perforations were more probably due to atmospheric agency.

3. "On the Parallel Roads of Glen Roy." By Prof. James Nicol, F.R.S., F.G.S.

The author briefly noticed the two principal hypotheses which have been advanced to account for the formation of these terraces, and asserted his own belief in their marine origin. He rested his argument against the hypothesis of their lacustrine origin chiefly on the ground that if their formation be due to successive periods of repose alternating with sudden drainings of a lake occupying the present valley of Glen Roy, we ought still to find traces of the violent *débâcles* occasioned by these drainings, or of large rivers in the gorges through which the waters must have flowed. The author stated that no such appearances presented themselves; on the contrary, he adduced certain characters exhibited by these gorges, which, he considered, were strongly in evidence of long-continued sea-action, and seemed to indicate that the gorges in question had been occupied, at the time of the formation of the terraces, by arms of the sea.

Mr. H. M. JENKINS objected to the supposition of the sudden alteration in the level of the water adopted by the author. He thought the gradual sinking of the water was quite compatible with the formation of the roads. He instanced the formation of terraces in gravel-pits filled with water.

Sir H. JAMES announced that the Ordnance survey of the district in question was now complete.

4. "On Beds of supposed 'Rothliegende' age near Knaresborough." By J. Clifton Ward, Esq., F.G.S.

The author called attention to certain beds occurring in the neighbourhood of Knaresborough, especially at Plumpton, either of a coarse and conglomeratic structure, or consisting of sandstones or sandy shales. These beds have been regarded by some as belonging to the "Rothliegende" series; by others as belonging to the Millstone Grit. The chief arguments in favour of their belonging to the Millstone-Grit are, as stated by the author:—1. Their similarity to true Millstone-Grit beds; 2, their occurrence in

a Millstone-Grit area; 3, their conformity to the underlying Millstone-Grit rocks, and the unconformity of the overlying Magnesian Limestone; 4, their containing plant-remains similar to those of the Millstone-Grit; 5, their colour. Their purplish tint, and resemblance to certain German "Rothliegende" conglomerates, are the only characters which seem to unite them with beds of that age.

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#### XLIV. *Intelligence and Miscellaneous Articles.*

##### RESEARCHES ON SOLAR RADIATION.

BY MM. P. DESAINS AND E. BRANLEY.

WE have the honour of laying before the Academy an account of some experiments we have made this summer with the view of solving certain questions relative to solar radiation. These questions are:—

1. The influence of altitude on the intensity of the calorific action exerted by the sun at points whose projections on the ground are not very distant from each other.

2. The influence of the altitude of a point on the composition of the sun's rays which strike it.

3. How the composition of the heat sent to the same point varies with the height of the sun above the horizon when the quantity of vapour contained in the air does not vary during the experiments.

The apparatus we used had been previously constructed by M. Duboscq and M. Ruhmkorff under the direction of one of us (M. Desains). We will give a rapid description of it.

The first part is a parallactic apparatus consisting of a Silbermann's heliostat, the clockwork of which moves not as usually a mirror, but a tube whose axis follows the sun in its daily course.

This tube has a double envelope; and its inner chamber has a thermoelectric pile, on one face of which the sun's rays strike when a stop is removed which closes the corresponding end of the tube in which it is placed. In the tube itself, between the end and the pile, diathermanous screens may be placed so as to act on the incident rays.

The second apparatus used in our researches is more easily transportable than that we have just described. One of its principal parts is a leg with a double movement, which supports at the top a copper tube about 0.06 metre in length by 0.05 metre in diameter. The movements with which the apparatus is furnished render it possible to keep its axis continually pointing to the centre of the sun; and, thanks to a system of lateral sights, it is always possible to judge whether the condition is fulfilled.

In this tube or moveable support a thermoelectric pile may be fixed, provided with an envelope identical in all respects with that we have described above.

Instead of the pile it is sometimes advantageous to introduce the blackened bulb of a thermometer, the stem of which projects through an aperture constructed for that purpose.

It is also possible to introduce a spectroscope of a particular kind,

the object of which is to estimate the influence which various causes may have upon the position of maximum heat in the solar spectrum. We shall not give a detailed description of its construction, but merely remark that it consists essentially of a bent tube which has at one end a stop with a rectilinear aperture the width of which may be varied, in the middle a lens and a prism of rock-salt or of fluor-spar, and, finally, at the other end a line thermopile, the direction of which may always be made parallel with that of the spectral rays investigated. A micrometric screw renders it possible to move this pile slowly in a plane perpendicular to the axis of that part of the tube which supports it, and thus bring it into all parts of the spectrum.

Our experiments on the influence of altitude on the intensity and composition of the sun's heat were made simultaneously from the 8th to the 15th of last September—on the one hand at the Schweizerhof at Lucerne, and on the other at the Rigi-Kulm hotel, about 4756 feet above the lake. They have shown that, at the same time and all other things being equal, the solar radiation was more intense on the top of the Rigi than at Lucerne, but that it was far less transmissible through water and alum. The following are some of the numbers:—

On Monday, September 13, at 7<sup>h</sup> 45<sup>m</sup> A.M., in fine weather, the sun's rays at the top of the Rigi produced a deflection of 27°·2 of the needle of the instrument which was set up there. At Lucerne, at the same time, the deviation was 30°·3. Now the second apparatus was more delicate than the other in the ratio of 277 to 204; this we determined on the spot at Lucerne. Hence the Lucerne apparatus, if it had possessed the same delicacy as that on the Rigi, would have given 22°·5 when the latter gave 27°·2. Expressing these results in hundredths, we come to this result,—that on Monday the 13th, at a quarter to 8 in the morning, the sun's rays, in traversing under an angle of about 70° the layer of air between the level of the top of Rigi-Kulm and that of Lucerne, experienced a loss of 17·1 per cent.

The transmissibility of the radiation was less on the Rigi than at Lucerne. Through a glass trough 0·08 metre in length, full of water, the rays on the Rigi passed in the proportion of 0·685, and at Lucerne in the proportion 0·73. Many other experiments made, on the 10th, 12th, 13th, and 14th, led to results which agree with those we have just announced.

In the experiments of the 13th of September, the tension of aqueous vapour in the air on the top of the Rigi was 0·0063 metre, and at Lucerne 0·0086. We cannot, however, conclude thence that the mean tension of the aqueous vapour contained in the layer of air between the two stations is the mean of these numbers; for the tension on the Rigi close to the ground might be very different from what it was in air at the same altitude but at a distance of several hundred yards from the mountain.

The apparatus, the use of which has led us to the preceding results, may be applied without difficulty to determine the changes which the daily course of the sun produces in the intensity of the heat which it sends to a given point. This question has been treated by M.

Pouillet; we have not touched it; but we have made a tolerably numerous series of experiments to ascertain how the nature of the sun's heat varies with the time of day—that is, with the thickness of the layer of atmosphere traversed, and with the quantity of water which this layer contains. A first series of experiments was made between the 15th of July and the 15th of September, on fine days in which from 7<sup>h</sup> 30<sup>m</sup> in the morning to midday the atmospheric conditions underwent no great changes. Under these circumstances, at Paris as well as Lucerne, the sun's heat always appeared more transmissible through water and through alum in the morning than it was at midday. Thus, at Lucerne, on Monday, September 13, at 7<sup>h</sup> 30<sup>m</sup> A.M. the sun's heat passed through a glass trough containing a layer of water 0.004 metre in thickness in the proportion of 0.755; at noon the transmission through the same trough was not more than 0.71. In August, at Paris, we obtained still more striking differences. In October, on the contrary, in mornings in which the temperature was near zero in the early part of the day and rose much towards the middle, the differences in transmissibility were no longer apparent; and this is readily intelligible—the differences between the thicknesses of air traversed by the rays in the course of the experiments being less than in summer, and their influence lessened by a considerable increase in the quantity of aqueous vapour.

Our spectroscopic observations have led to results which tend to confirm the preceding. At Lucerne, on the 13th and 14th of September, the maximum seemed at midday a little more distant from the red than in the morning. In October, on the contrary, its position seemed constant, as did also the transmissibility of the rays through troughs full of water.—*Comptes Rendus*, November 29, 1869.

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AVOGADRO'S LAW DEDUCED FROM THE FUNDAMENTAL CONCEPTION OF THE MECHANICAL THEORY OF GASES. BY ALEX. NAUMANN.

Avogadro's law, that equal volumes of different gases at the same temperature and pressure contain the same number of molecules, is probably regarded by the majority of scientific chemists as the most certain basis for fixing molecular and atomic weights, which enable chemical compositions and chemical processes to be expressed in the simplest and most natural manner. Hence it may be desirable to furnish a proof that the same law may be deduced as a necessary consequence from a totally different basis and in a totally different manner.

In the development of the mechanical theory of gases, Avogadro's law has been used \* as a simple and probable assumption. But it may also be deduced as a necessary consequence from the well-founded assumption that the molecules of gas are very small as compared with their mean distances from each other, and behave like elastic spheres,

\* Compare Clausius, *Pogg. Ann.* 1857, p. 367.



and have a progressive motion\*—if taken in conjunction with certain facts confirmed by experiment.

In accordance with the said assumption, the pressure of the gas is found to be the sum of all the impacts which the molecules of gas effect upon the adjacent bodies in virtue of their progressive motion. The pressure on the unit of surface will depend, therefore, (1) on the mass of each individual molecule of gas, (2) on the velocity of the molecules, (3) on the volume of the gas, (4) on the number of molecules.

Let us, on the one hand, take the mass of a molecule as  $m$ , and its velocity  $c$ ; on the other, the mass  $M$  and the velocity  $C$ . Then the force of the individual impacts is directly as the masses and the velocities; it is therefore proportional to the product of the two so-called magnitudes of motion—that is,  $= \frac{mc}{MC}$ . But the number of impacts effected by a molecule in equal times is proportional to its velocity—that is,  $= \frac{c}{C}$ . Hence the pressures as regards masses and velocities are

$$\frac{p}{P} = \frac{mc^2}{MC^2} = \frac{\frac{mc^2}{2}}{\frac{MC^2}{2}} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Hence, if we suppose that in the volume  $v$  there are  $n$  molecules at equal distances and in a state of rest, in the unit of volume there are  $n_1 = \frac{n}{v}$  molecules; hence in a path equal to the unit of length there

are  $l = \sqrt[3]{\frac{n}{v}}$ , and in the unit of surface  $f = \left(\sqrt[3]{\frac{n}{v}}\right)^2$  molecules. If,

now, the number of molecules being the same, the volume  $v$  is changed to the volume  $V$ , in the unit of volume there will be

$n_2 = \frac{n}{V}$  molecules, and there are therefore in the unit of length

$L = \sqrt[3]{\frac{n}{V}}$ , and in the unit of surface  $F = \left(\sqrt[3]{\frac{n}{V}}\right)^2$  molecules. In

a state of motion the pressure upon the unit of surface must be pro-

\* In favour of this conception of the gaseous condition, we have, as coming most immediately within the cognizance of the senses, the recent microscopic observation (Fick, *Die Naturkräfte in ihrer Wechselbeziehung*, Würzburg, 1869, p. 27) that fine particles floating in the air are animated by a tremulous motion, and, further, the observation (O. E. Meyer, *Pogg. Ann.* vol. cxxv. pp. 177, 401, 564) that the friction of gases is independent of the density, but increases with the temperature, as is required by a progressive rectilinear molecular motion increasing with the temperature (*loc. cit.* pp. 584-598, compare also p. 179).

portional to the number of molecules impinging upon the unit of surface in equal times; that is, it must, on the one hand, be proportional to the molecules present in it in a given moment (because these, when in motion, impinge simultaneously upon it), and, on the other, proportional to the number of molecules in the unit of length (for the surface will be the more frequently struck the smaller the distance of the molecules, or, in a certain sense, of the molecular layers). Hence the pressures at any time are

$$\frac{p}{P} = \frac{fl}{FL} = \frac{\left(\sqrt[3]{\frac{n}{v}}\right)^2 \sqrt[3]{\frac{n}{v}}}{\left(\sqrt[3]{\frac{N}{V}}\right)^2 \sqrt[3]{\frac{N}{V}}} = \frac{\frac{n}{v}}{\frac{N}{V}} = \frac{v}{V}. \quad \dots \quad (2)$$

From equation (2) we get also  $\frac{p}{P} = \frac{n}{v} \cdot \frac{V}{N} = \frac{n_1}{n_2}$ , As this ratio  $\frac{n_1}{n_2}$  of the molecules contained in the unit of volume is equal to the ratio  $\frac{n}{N}$  of the number of molecules contained in equal volumes  $v=V$ , we have for equal volumes

$$\frac{p}{P} = \frac{n}{N}. \quad \dots \quad (3)$$

If, then, both the volumes and the molecular numbers are different, we get, by combining equations (2) and (3),

$$\frac{p}{P} = \frac{nV}{Nv}. \quad \dots \quad (4)$$

By combining equations (4) and (1) we have, further,

$$\frac{p}{P} = \frac{nV \cdot \frac{mc^2}{2}}{Nv \cdot \frac{MC^2}{2}}. \quad \dots \quad (5)$$

From Mariotte and Gay-Lussac's law we have, for the same gas at constant volume,

$$\frac{p_1}{p_2} = \frac{n_1}{n_2} \cdot \frac{273+t_1}{273+t_2} = \frac{n_1 T_1}{n_2 T_2}, \quad \dots \quad (6)$$

in which the ratio of the numbers  $n_1$  and  $n_2$  of the molecules contained in equal volumes represents at the same time the ratio of the densities.  $T_1$  and  $T_2$  denote what are called the absolute temperatures, counted from  $-273^\circ \text{C}$ .

But if, for equal volumes of the same gas,  $c_1$  and  $c_2$  denote the velocities corresponding to the absolute temperatures  $T_1$  and  $T_2$ ,

equation (5) is changed into

$$\frac{p_1}{p_2} = \frac{n_1 \cdot \frac{mc_1^2}{2}}{n_2 \cdot \frac{mc_2^2}{2}} \dots \dots \dots (7)$$

From equations (6) and (7) we have

$$\frac{\frac{mc_1^2}{2}}{\frac{mc_2^2}{2}} = \frac{T_1}{T_2} \dots \dots \dots (8)$$

In mixing at the same temperature (*e. g.*  $T_2$ ) different gases which do not act on each other, this temperature remains unchanged, independently of the ratio of the quantities of the mixed gases and their volumes. Hence at the same temperature the *vis viva* of the molecular motion must be equal even in different gases; that is,

$$\frac{mc_2^2}{2} = \frac{MC_2^2}{2} \dots \dots \dots (9)$$

From equations (8) and (9) we have

$$\frac{\frac{mc_1^2}{2}}{\frac{MC_2^2}{2}} = \frac{T_1}{T_2} \dots \dots \dots (10)$$

Replacing in equation (5) the ratio of the *vis viva* of the molecules according to equation (10) by the ratio of the absolute temperatures, we have

$$\frac{p}{P} = \frac{nVT_1}{NvT_2} \dots \dots \dots (11)$$

Putting finally in equation (11)

$$p=P, \quad T_1=T_2, \quad v=V,$$

we have also

$$n=N;$$

that is, *for the same pressure and temperature equal volumes of different gases contain the same number of molecules.*—*Berichte der Deutschen chemischen Gesellschaft*, December 24, 1869.

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XLV. *On the present state of the question as to the Limits of the Jurassic and Cretaceous Periods.* By F. J. PICTET\*.

UNTIL within the last few years all geologists have accepted the limits of the Cretaceous and Jurassic formations as they were laid down for the deposits of England and the North of France. No one ventured to suppose that things may possibly have gone on otherwise than in the Anglo-French basin. We were accustomed to assume that the Kimmeridgian and Portlandian stages everywhere formed the upper limit of the Jurassic period, and the Neocomian stage (including in this the Valangian) the lower boundary of the Cretaceous period, these limits being at some points rendered still more distinct by the existence of intermediate freshwater deposits (Wealden).

New discoveries and investigations have thrown some doubt upon this uniformity; and the general question, originating almost at the same time in Germany and in France, has acquired an importance and development which seem to me to render it worthy of the attention of the Society. I have therefore thought that I might render a service to some of our colleagues by summing up as briefly as possible the numerous discussions which are now scattered through several periodicals, and which it is not always easy to coordinate.

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The first work known to me which directly brought in ques-

\* Being a Report made to the Session of 1869 of the Swiss Society of Natural Sciences. Translated by W. S. Dallas, F.L.S., from a separate impression, communicated by the author, from the *Bibliothèque Universelle*, vol. xxxvi. pp. 224-246.



tion the limits of the Jurassic and Cretaceous periods is a memoir by Oppel\*, the purpose of which was to show that, between those Jurassic stages which have been regarded as the uppermost, and the lowest Cretaceous stage, there exist thick strata characterized by a very rich fauna. These deposits had, indeed, been noticed by several geologists, and especially in Switzerland by MM. Studer, Brunner, and Fischer-Ooster, and in Germany by MM. Suess, von Hauer, Peters, &c.; but Oppel, by a detailed comparison of the facts, gave them quite a new significance. He grouped the whole of this series of deposits under the name of the *Tithonic stage*, which he limited below by the Kimmeridgian stage on which it often rests, and above by the Neocomian stage. He was inclined to regard it as the upper term of the Jurassic series. We have not here to discuss the value of this new name, which seems to us to represent rather too large a totality†. The author himself moreover announced his Tithonic stage as constituting a provisional association, destined to be subdivided hereafter. Considered generally and rather vaguely, it represents a right idea by which science has made a positive advance. Applying these data to the special case of the limestones of the Porte-de-France, to which we shall refer by-and-by, Oppel was the first who conceived the idea of reducing the age of these beds, by raising them in the chronological series of formations.

Whilst Oppel sought in this way to raise the superior term of the Jurassic series, M. Hébert, taking up another phase of the question, was inclined to lower the inferior term of the Cretaceous series. For some years the learned Professor of the Sorbonne had remarked that, below the bed designated by M. Lory the *Lower Neocomian marls*, which passed as the oldest Neocomian bed, there were some limestones possessing the lithological appearance of the subjacent Jurassic rocks, and nevertheless containing Neocomian fossils (*Ammonites macilentus* &c.). In 1861 our colleague, Professor Studer, accompanied M. Hébert in a journey the principal result of which was to show the existence of this new term in the Cretaceous series. At first this discovery attained but little publicity beyond that which was given to it by general and personal statements made in the laboratory of the Sorbonne to all the geologists and palæontologists who visited those interesting collections.

I myself knew of it by conversations with M. Hébert; and after having often pressed him to give a real publicity to these

\* "Die Titonische Etage," in the *Zeitschrift der Deutschen Geologischen Gesellschaft* for 1865. Translated and abridged by M. de Loriol, *Bibl. Univ.* (*Archives*) for January 1866.

† MM. Benecke and Zittel have already proposed to exclude from it a certain number of local beds, such as those of Solenhofen &c.

facts, I obtained for the *Bibliothèque Universelle* a notice\* which formed the primary basis of the discussions in which I have taken part, and which have repeatedly occupied the Geological Society of France.

The principal object of this note was to establish, in the first place, the existence of the inferior limestones already mentioned, which carry the origin of the Cretaceous period to a more ancient epoch than had been supposed. Then passing to the investigation of the limestones of the Porte-de-France, the section of which was well known from the excellent works of M. Lory, M. Hébert came to the conclusion, from exclusively palæontological considerations, that these limestones should be ascribed to the Neocomian period, and not to the Oxfordian stage, as they usually had been.

Our learned friend founds his conclusions upon the investigation of fossils communicated by MM. Lory and Chaper, and cites, as an important character of these beds, the presence of *Terebratula diphya*, which, according to him, is incontestably Neocomian and only forms a single species; for, he says, the most careful examination does not enable us to find the smallest constant specific character between this and *T. diphyoidea*†.

This novel and bold opinion as to the age of the *Terebratula diphya* limestones of the Porte-de-France was received with some astonishment, and became immediately the subject of discussions. I must explain in a few words how I was led to mix myself up in them. I one day received a visit from Brother Euthymus, who brought me a series of fossils from Berrias, in which I speedily recognized a fauna with Neocomian characters, but consisting for the most part of unknown species. Brother Euthymus was kind enough to accompany me into the department of the Ardèche; and then, thanks to the assistance of the authorities of Privas, and to investigations made at Berrias itself under the auspices of M. de Malbos, I was able to publish a monograph on the Limestone with *Terebratula diphyoidea* of Berrias‡, and thus to add, to the first discovery of M. Hébert, the description of a Neocomian fauna of greater antiquity than any of those then known.

\* Hébert, "Sur les limites de la période Jurassique et de la période Crétacée, et spécialement sur les calcaires à *Terebratula diphya*," *Bibl. Univ., Arch.*, August 1866. A similar notice was published in 1869 in the *Comptes Rendus*, vol. lxiv. p. 1053; this was completed by a second note in the *Bull. de la Soc. Géol.* 1867, vol. xxiv. p. 389.

† In his subsequent works, M. Hébert has been less positive; and if I here repeat his first assertion, which has since been much softened, it is solely to show the necessity of a monograph upon the *Terebratulæ* of the *diphya*-group.

‡ *Mélanges paléontologiques*, 2<sup>me</sup> livr. vol. i. p. 41.

It then became necessary to have a better knowledge of the *Terebratulæ* of the *diphyæ*-group\*, inasmuch as arguments, in my opinion erroneous, were drawn from them to justify exaggerated associations. I distinguished several species, the characters of which are, in my opinion, perfectly clear; and I showed that these species, although but little apart as regarded their age, may serve to characterize certain distinct beds.

These two preparatory investigations being made, I also took up the study of the limestones of the Porte-de-France, and I summed up my opinion nearly as follows†, taking as my basis, like M. Hébert, the classical memoirs of M. Lory.

The section of Grenoble and of Aizy is composed of three principal stages.

The base is occupied by a Jurassic fauna, analogous by its fossils to that of the beds of Baden in Argovia, or to the fauna of *Ammonites tenuilobatus*. This fauna, which in Germany has often the same geological relations with the Stramberg limestone, is regarded by M. Hébert as Oxfordian, and by MM. Benecke, Zittel, &c. as Kimmeridgian‡.

Above this, and from the appearance of *Terebratula janitor* (formerly confounded with *T. diphyæ*), there is a limestone which, by its fossils, perfectly resembles the Stramberg limestone, with which it is incontestably contemporaneous. With this limestone there is associated at Aizy a coralline breccia which seems to contain a mixture of Jurassic and Cretaceous species.

The top of the section (cement-marls) is formed by beds identical with the limestone of Berrias.

Now, where is the limit between the two periods? This is what remains to be fixed. I supposed a line, A, between the first formation and the second, and a line B, of less importance, between the second and the third. I showed that the limit was perhaps at the line A, less probably at the line B, and perhaps, again, between the two. M. Hébert accepts the line A. We shall see hereafter the difficulties which surround the solution of this question.

It may be added, for the comprehension of the relations of this section with our faunas of the Swiss Jura, that the limestone of

\* *Mélanges paléontologiques*, 3<sup>me</sup> livr. vol. i. p. 143.

† Ibid. 4<sup>me</sup> livr. vol. i. p. 205. See also "Nouveaux documents sur les limites de la période Crétacée" &c. (*Bibl. Univ., Arch.*, June 1867), and "Notice sur les Calcaires de la Porte-de-France" (ibid. October 1867).

‡ I have adopted this last opinion, but I must add that the determination of the exact age of the Jurassic bed has been to me but a secondary question, of which I leave the discussion to others. It must be remarked that the facies of these faunas varies much in different places, and renders necessary a long and complicated investigation, for which I do not possess the requisite materials.

Berrias probably represents an horizon immediately below our Valangian. In the Department of the Bouches-du-Rhône M. Coquand has found this Berrias limestone immediately covered by the bed with *Natica leviathan*, a fossil characteristic of the Lower Valangian.

Thus it will be seen that the great features of the question were pretty well sketched out in these first works. It remains for us to see what has been its subsequent development.

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Every one now seems to be nearly agreed as to the facts which we have just related. The succession of the formations is not contested; and it is useless to refer here to some divergences of detail which do not in any way affect the general solution of the question\*.

Numerous works published of late years show that this succession of faunas has been the same at many points distant from each other. MM. von Hauer, Suess, Zittel, Benecke, Mojsisovics, Neumayr, &c. have ascertained facts remarkably in accordance with those which I have just indicated; and we may regard it as a settled point that from the Carpathians to the Mediterranean, wherever the Tithonian formation is met with, the stages are usually arranged in conformity with the following section:—

1. Neocomian stage, properly so called.
2. Valangian stage, and marls with *Belemnites latus*.
3. Berrias limestone.
4. Tithonian stage.
5. Bed with large *Aptychi*.
6. Jurassic fauna with *Ammonites tenuilobatus*.

The principal question, that of the limit of the Jurassic and Cretaceous periods, may therefore be presented here as the alternative between the following solutions.

The line of separation may be the line A of my memoir—that is to say, the line passing between Nos. 4 and 5.

It may be drawn a little lower—that is to say, between Nos. 5 and 6.

This line of separation may also be the line B of my memoir, and pass between Nos. 3 and 4.

\* In particular M. Hébert and myself have not always interpreted the species in the same way. For my own part I persist in the greater part of my determinations, but I must remark that these differences of appreciation cannot at all modify the results indicated. Moreover it would be easy for me to show that the signification of my assertions has often been too much extended. Thus, when I proved (with MM. Bayle and Chaper) that *Ammonites Stazyscii* and *A. Liebigi* are not *A. Grasianus* and *subfimbriatus*, I was told in reply that these two species pass into the Neocomian, which I have never denied; and so forth.



To these possible solutions, which have already been discussed, two others may be added, which we shall analyze further on, and which tend to acquire a certain amount of probability.

May it not pass through the middle of No. 4, separating a Jurassic from a Cretaceous Tithonian?

And, lastly, is not this line an illusion? and is it not possible that there has been no interruption between the two periods?

Before going into details, I must say that it is here especially that, under some points of view, I have diverged from M. Hébert; but our divergences have been considerably exaggerated, and relate especially to a question of method. Now the more we approach the solution, the more will the facts acquired predominate over this question of method; and when the truth shall be rendered incontestable, it will be of less importance to know by what course we have arrived at it.

M. Hébert now declares\* that he has occupied himself only with the question of the Porte-de-France, and that the bed with *Terebratula diphya* (*janitor*) is incontestably Neocomian†. He retains some doubts with regard to the Coralline breccia superposed upon or associated with the limestone of Aizy, of which I have already spoken.

The opinion which I put forward in my first note, and which I have since constantly maintained, is that the question must be solved by a general comparison of the strata in dispute over a large geographical space. The solution must be capable of application not only to the south of France, but also to the Carpathians, to Italy, to the Tyrol, to Spain, to Algeria, &c.; we may consequently expect that it will be very complex.

I must remark, indeed, that the Department of the Isère alone, notwithstanding the excellent works of M. Lory, does not furnish sufficient documents. M. Hébert and myself have worked upon the same materials, which are but scanty, and among which there are numerous species of uncertain origin. We have deduced from them, I believe, all that can be deduced; we may, perhaps, even be accused of having acted sometimes with a little temerity. For absolute certainty, more well-characterized species found *in situ* in a very clear section would be necessary. I continue to be profoundly convinced that the Lithographic limestones of the Porte-de-France and Aizy must follow the lot of those of

\* Geological Magazine, July 1869, vol. vi. p. 301.

† M. Hébert would perhaps have done better had he made use of the word *Cretaceous*; for however possible we may consider it that the Tithonian stage forms the oldest member of the Cretaceous period, we think we should refuse the name of *Neocomian* to a formation so profoundly different from the stage which bears this name, and which has not 5 per cent. of species in common with it. But this is a secondary question, and we shall proceed with the discussion as if M. Hébert had employed the broader word.

Stramberg, and that their true value can only be elucidated by a detailed investigation, of which I must now indicate the difficulties.

We already possess numerous documents upon the limestone of Stramberg. From the special point of view which interests us, supposing all the determinations to be accurate, and assuming all the fossils described to be exactly contemporaneous, what we know may be summed up in the following facts, which appeared, a few months ago, to be the last words on the question, and which, in order to preserve a chronological order, I must analyze here, to discuss and modify them hereafter.

The Cephalopoda, which have just been completely studied by M. Zittel\*, number fifty-five species, of which fifty are new. The five others have their analogues in the Cretaceous period. Not one occurs in the Jurassic stages. This proportion, if it were constant, would solve the question in favour of the line A; but the other documents are far from leading to the same result.

The Brachiopoda, which have likewise been carefully investigated, first by M. Zeuschner† and then by M. E. Suess‡, one of the most competent palæontologists in this department, give a different result. Of thirty-eight species described, twenty-six are new; none pass into uncontested Cretaceous stages; eleven recur in the Jurassic period; and one (*Terebratula janitor*, Pict.) is particularly valuable because it also occurs at the Porte-de-France, and in some other deposits, of which it seems to show the analogy with the limestone of Stramberg.

The Gasteropoda are less known. M. Zeuschner§ and M. Peters|| have described the *Nerinae* of Inwald, a deposit which passes as contemporaneous with that of Stramberg. They have an essentially Jurassic facies.

This is the case also with the other Gasteropoda and the Acephala, at least judging from still unpublished comparisons made in the magnificent collection of the Museum at Munich, to such an extent that M. Zittel has concluded from these first data that the Tithonian stage presents a fauna identical with those of Wimmis and of Mont Salève, which have hitherto been ascribed to the Corallian stage.

Such is the principal difficulty. If all these facts be correct, we must assume that on the confines of the two periods there are mixed faunas, and that the Cretaceous species, having successively appeared, have gradually modified the Jurassic faunas.

\* *Palæontologische Mittheilungen*, vol. ii. part 1.

† *Palæontologische Beiträge zur Kenntniss des weissen Jurakalks*.

‡ In von Hauer's *Beiträge zur Palæontologie von Oesterreich*, vol. i. p. 15.

§ "Ueber den Nerineen-Kalk von Inwald," Haidinger's *Abhandlungen*, vol. iii. (1849).

|| "Ueber Nerineen," *Sitzungsber. Akad. Wiss.* vol. xvi. (1855).

But the admission of such a fact, opposed to the systematic teaching of the schools, cannot be accepted until it has been freed from all chances of error. It is particularly necessary to ascertain if those which have been referred to the Stramberg limestone really belong to it, and whether they have really lived together at the same time and constituted a single fauna, as is required by the conclusion which we have just indicated.

Now it must be admitted that the most recent investigations have rendered this contemporaneity to a certain extent doubtful. It appeared very probable a few months ago; but now it is no longer certain; and we must explain, before going further, the arguments which may be adduced to modify the ideas previously put forward. But, as will be seen hereafter, the conclusions with which we can replace them present themselves under a curious and unexpected light, and raise questions of quite equal gravity.

The first doubts were set on foot by the study of the Swiss beds associated with the Stramberg limestone—that is to say, the Corallian stage of Wimmis and that of Mont Salève. These deposits, notwithstanding evident relations with the Tithonian stage, are nevertheless deficient in several characters which would render this analogy incontestable. Thus neither at Wimmis nor at the Salève has *Terebratula janitor* been found, any more than any of the Cephalopoda of Stramberg. If we might suppose that the German geologists have combined into one two distinct beds, of which the more recent is characterized by this *Terebratula* and the Cephalopoda, and the deeper one by another set of fossils, would it not be possible that we should find with us only the inferior fauna, designated by some geologists the fauna of *Terebratula moravica*, and which would constitute the last term of the Jurassic period, whilst the true Stramberg would form the base of the Cretaceous formations. M. Hébert gives us the good news that he has commenced a comparative investigation of all these faunas.

These doubts are in great part confirmed by a recent note by M. Coquand, "On the Stages of Provence"\*. The analysis of this important document would lead us too far. Its principal result is, that in this region the fauna of *Terebratula moravica*, entirely destitute of Cephalopoda and probably identical with that of the Coralline limestones of the Salève and Wimmis, is covered by beds containing Kimmeridgian and Portlandian Ammonites, and consequently is evidently Jurassic. These facts, at least as far as we can judge of them from a first sketch, seem to indicate that there are two different orders of succession according to the

\* *Bull. de la Soc. Géol.* 1869, vol. xxvi. p. 100. See also in the same Number a note by M. Hébert, p. 131.

regions. In one (Provence, Salève, Wimmis) the stages are nearly conformable to what we find in the rest of France, and terminate in the littoral Neocomian; the limits of the Jurassic and Cretaceous appear there to be distinct. In the other, comprised between the Carpathians and Italy (with a portion of the French Alps, of the Department of the Isère, &c.), the Tithonian stage rules on the confines of the two great periods.

It is to this latter form of succession that we have to direct our attention. In this affairs have taken place in a very different manner from that which characterizes the Anglo-French basin. To be convinced of this, we have only to study the recent investigations of M. Zittel\* in the central Apennines, and those of M. Neumayr† in the Carpathians. These works seem to prove that the Tithonian stage must be divided into two, neither of which, indeed, is perfectly identical with the fauna of *Terebratula moravica*, for both of them are rich in Ammonites.

In the central Apennines the Tithonian stage is strongly developed and abounds in fossils, but, according to M. Zittel, it presents peculiar palæontological conditions. Of forty-five species, consisting in great part of Cephalopoda, there are thirty-one in common with the *Diphyia*-limestone of the Tyrol, and twenty-nine with the breccia of Rogoznik. Its analogy with Stramberg is less, and supported by only thirteen species.

From this and from different comparisons, M. Zittel infers that the Tithonian stage, as we have just stated, forms two divisions. The superior of these is the Stramberg limestone, the fauna of which has more analogy to Cretaceous than to Jurassic forms. It corresponds to the coralline beds of the Wolfgang lake, and also to the lithographic beds of the Porte-de-France and of Aizy.

The lower one, which nevertheless evidently forms part of the same whole, includes the greenish marble of the central Apennines, the *diphyia*-limestone of the Tyrol, and the Klippenkalk of Rogoznik. The fauna has a more Jurassic character than that of the other division. Out of forty-five species only one is Cretaceous, and at least four are Jurassic.

M. Neumayr has studied the formations situated between Rogoznik in Galicia and the county of Saros in Upper Hungary, along a long and narrow band. These formations present an interesting and instructive series of faunas, which, like the investigations of M. Zittel just cited, show the extreme difficulty that there is in finding a limit between the Jurassic and Cretaceous periods in the vicinity of the Tithonian stage.

\* M. Benecke's *Geogn.-pal. Beiträge*, vol. ii. part 2. Munich, 1869.

† "Ueber Dogger und Malm im penninischen Klippenzug," *Verhandl. der K. K. geol. Reichsanstalt*, 1869, No. 5.



We shall not refer here to the lower deposits, which have nothing to do with our subject, namely the beds with *Ammonites opalinus*, those with *A. Murchisonæ*, and the Crinoidal limestoue. Those which interest us are:—

1. The lower Tithonian fauna, which may be subdivided into two:—

A. The fauna of the Czorsztyń limestone, from which M. Neumayr cites twenty-five species. Of these, twelve also occur in the blue marble of the Apennines. The general fauna especially approaches the Jurassic fauna known under the names of the fauna of *Ammonites acanthicus* and *A. tenuilobatus*. Some species are the same as those of Stramberg (*A. silesiacus*, *Kochi*, *ptychoricus*, &c.). Only one persists into the true Neocomian (*A. quadrisulcatus*). The perforated *Terebratula* are *T. Catulloi*, Pict., and *T. sima*, Zeuschn.

B. The true Tithonian fauna, containing thirty-eight species of Cephalopoda, of which sixteen reappear in the breccia of Rogoznik, seventeen in the blue marble of the Apennines, and only eight at Stramberg. *Terebratula Catulloi* and *T. sima* persist.

2. A white limestone which has been found only at two points in the south-west, and which corresponds to the true Stramberg limestone. Of fourteen determinable *Ammonites*, twelve also occur in the latter deposit; of these seven are common to Stramberg and Rogoznik, and only one is peculiar to the latter stage.

We must add to these memoirs a very interesting investigation of the fauna of *Terebratula janitor* made in the environs of Palermo by M. G. G. Gemellaro\*. According to his researches, the grey limestone of Monte Pellegrino contains, together with several *Ammonites* characteristic of Stramberg, an abundant fauna of Gasteropoda, among which are several of the *Nerinea* of Inwald, and also *Terebratula janitor* associated with *T. moravica*! Before taking all the advantage of these facts that we may hope from them, we must, however, wait for the author's final conclusions. It is not impossible, from some statements, that he also may recognize two distinct stages in this deposit.

Such are the principal facts with which we are at present acquainted, and upon which we can found our judgment. I may be permitted to point out that the question has become singularly modified, and that I was not wrong when I called upon paleontologists not to overstep the proofs furnished by the facts, and to postpone a definitive solution. I am still of the same opinion, although the reasons for waiting have become less forcible, and we are now much nearer the solution.

\* *Studi paleont. sulla fauna del Calcarea a Tereb. janitor del norde di Sicilia*. 4to. Palermo, 1868 et seqq.

The following, it seems to me, is the mode in which we may sum up the concordant portions of the documents above cited.

Leaving on one side, for reasons already indicated, the stages of Provence, Mont Salève, and Wimmis\*, and applying what follows only to countries in which the Tithonian stage is well developed, we find in ascending order:—

1. The fauna of *Ammonites tenuilobatus* ;
2. The fauna of the Lower Tithonian, represented especially by Rogoznik, the blue marble of the Apennines, and probably by the limestone with *Terebratula diphya* of the Tyrol ;
3. The fauna of the Upper Tithonian, or Stramberg limestone (*Tereb. janitor*) ;
4. The Lower Neocomian stage, and particularly the limestone of Berrias (*Tereb. diphyoidea*).

Nos. 1 and 2 have dominant Jurassic characters ; No. 3 is rather cretaceous ; No. 4 has most of the normal characters of the latter epoch.

Those who seek for the line of separation between the two great periods place it now between Nos. 2 and 3. The perforated *Terebratulæ*, then, give the following result: the *Terebratula* with a small aperture (*T. diphya*, *Catulloi*, &c.) belong to the most recent Jurassic deposits ; the *Terebratula* with a large aperture (*T. janitor* and *diphyoidea*) are Cretaceous.

But this solution, which is apparently so simple, raises a great question, almost identical with that which we indicated above. The four stages above cited are bound together by great palæontological analogies. Several species incontestably pass from No. 1 to No. 2, from this to No. 3, and from No. 3 to No. 4. Nos. 2 and 3 especially, which would be separated by the line of demarcation of the periods, have about one-third of their species in common. This line would therefore be a very weak boundary, and we should have to admit that in this Tithonian basin the separation of the Jurassic and Cretaceous periods was singularly compromised.

In the presence of such a result, so much opposed to generally received notions, we can only repeat what we have said in previous works. Let us wait for fresh facts, and suspend all definitive judgment until we become acquainted with new sections, and new local faunas can be adduced to confirm or invalidate what our present results seem to force upon our acceptance.

We may especially hope soon to have a satisfactory description

\* These coralline limestones of Wimmis and Mont Salève belong, as I have already said, to the Upper Jurassic, and are in all probability the equivalents of the Inferior Tithonian stage. A good palæontological investigation is indispensable before we can appreciate their precise relations with the Tithonian stage of the Voirons, of Châtel-Saint-Denis, &c.

of the fauna of *Tereb. diphya* of the Tyrol, and of that which MM. de Verneuil and E. Favre have discovered in Andalusia. Both these are of great importance, and may furnish some of the elements that we require for the definitive solution.

In the meanwhile I will conclude with a few theoretical reflections on the manner in which we should receive and discuss these unexpected facts.

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Every one who seeks to enrich natural history with new facts labours more or less under the dominion of certain general and theoretical data, which cause him to see but one end to be attained and one course to follow. Few, indeed, escape this influence, which, it must be admitted, is most frequently a necessary excitant. But it is necessary to guard against the exaggerations of this principle, so as to avoid the danger of subordinating the results of direct observation to preconceived ideas. Allow me to call your attention now to some of these rocks which are to be shunned.

For my own part, I am quite agreed with those who believe that the development and the succession of organisms have always been and still are under the empire of perfectly regular laws. The function of science is to endeavour to discover these; and the further it advances the more brilliantly does this regularity manifest itself. I by no means take part with those who believe in irregularity and disorder, and whose tendency is to seek for apparent exceptions, the inanity of which is usually soon shown by the progress of observation.

But whilst taking this regularity as our basis, we must recognize the fact that the laws are much more complex than had been supposed. A few years ago we still believed in strongly marked lines, in great catastrophes, and in the sudden succession of faunas which had no material tie between them. We were brought up in these notions; and it has required all the teaching of numerous facts to show us that the faunas must have become modified more or less gradually, and with considerable slowness, under the influence of varied circumstances, like those which induce analogous effects in the existing seas. Now these circumstances are not identical at all times and in all places, which renders the probability that things took place exactly in the same manner in all geological basins infinitely small. The history of the succession of organisms must consequently be complex and modified by local facts; it is not probable that we shall be able to formulate it in a very simple manner applicable to all cases.

In presence of the question which now engages us, I think it

necessary to put prominently forward some special points of view.

The first to which I call attention relates to what is designated in geology under the name of *normal*. The earliest works which threw a great light upon the classification of strata were principally produced in England and the north of France. In the eyes of many naturalists the results obtained in the Anglo-French basin constitute normal geology, or the normal succession of the stages. The geology of other countries, where not strictly identical with this, only forms apparent exceptions which must be reduced to a signification in accordance with this normal state. To these naturalists the classification of the strata in the Anglo-French basin is the Sacred Ark, and the interpretation of facts observed outside of it should have for its object to make them fall within the frame of the picture. In this we easily recognize the tendency of a celebrated school, the chief representative of which, under this point of view, was the learned and lamented D'Archiac.

But why should the geologies of other countries not be equally *normal*? and why should they not have an equal right to fix the great divisions in the palæontological history of the globe? We may here refer to an excellent note by our learned colleague M. P. Merian\*, who, with his accustomed unerring scientific tact in placing his finger upon errors of method, has been so often able to give excellent advice to those geologists who have consulted him. In this note M. Merian shows that if it had chanced that the first development of geology had taken place in other countries, the bases of the classification of formations would have been in part different. There is no reason whatever why a Table constructed under these conditions should be less normal than that of the Anglo-French basin. In the case before us, in particular, we may say with certainty that if the first classifiers had commenced by the investigation of all that belt of land which is included between Galicia and the Mediterranean, passing through the Tyrol and the south-east of France, not one of them would have looked for the limits of one of the great periods in the neighbourhood of the Tithonian stage. The circumstance that the boundaries are very strongly marked in the Anglo-French basin does not imply of necessity that they are equally so in this other zone. These two orders of facts are not necessarily connected.

The second point of view upon which it is necessary to say a few words is with regard to the more or less complete inde-

\* *Ueber die Grenze zwischen Jura- und Kreide-Formation.* Basel, 1868. 8vo.



pendence of the stages. So long as we were acquainted only with the principal stages characterized by abundant faunas, we could believe in their complete independence; and this was certainly an important general fact; these successive well-defined faunas will always remain the necessary guide for the teaching and justification of our classification. But since many intermediate beds have been studied and traced to great geographical distances, some transitions have been met with. Fossils supposed to be very characteristic, have in some places preceded the fauna with which they principally lived, or have survived it a little. We have had to learn that affairs have gone on as would have been the case in the existing seas in consequence of gradual modification in the nature of the bottom, the depth, and the temperature. We have had to discover that, just as in our seas, the most marked faunas are separated by intervals in which the species are more or less mixed. Between the formations, as soon as they are known in all their details, there are analogous passages. Thus, taking for example the Cretaceous series, we know that whilst the principal formations were long regarded as characterized by perfectly special faunas, modern discoveries, while remarkably increasing the number of substages, have rendered most of the boundaries less definite than was supposed. The Valangian stage has many species in common with the Neocomian, and the latter with the Lower Urgonian. The Upper Urgonian has a part of the palæontological characters of the Lower Aptian. The Upper Aptian has some of those of the Gault. The Upper Gault, or Uraconian, has come in to diminish the differences which distinguished the Gault from the Cenomanian stage, &c.

I do not wish to exaggerate these facts, the importance of which is not sufficient to veil the existence of certain faunas recognized by every body; but they teach us that the stages can no longer be characterized by one or two isolated fossils, but only by the totality of the fauna.

May not what we have just said with regard to the stages into which the great formations are divisible, also apply to these formations themselves? What is the philosophical argument that leads us to regard these limits between the great formations as more strongly marked than others? Limits are inventions of science and useful as landmarks; but who has told us that there is an insuperable barrier between the last Jurassic and the first Cretaceous stage? Why should this barrier be so clear in the Anglo-French basin and not throughout? We should be a good deal embarrassed to furnish *à priori* a plausible reason for this.

Here again we are in accord with M. Merian's note. Our

learned colleague remarks with reason that a multitude of recent discoveries tend to render these limits obscure. Recent investigations establish passages between the Devonian and the Carboniferous. The Rhætic stage has filled up a part of the hiatus between the Trias and the Lias. The freshwater deposits lately discovered by M. Matheron strengthen the bond between the Cretaceous and Tertiary periods. The later Tertiaries can hardly be distinguished from the older Quaternary. Why should not the Tithonian stage in its turn diminish the value of the separation between the most recent Jurassic and the Lower Cretaceous stages?

We think, therefore, that in this important question it is more than ever desirable not to oppose to the facts which are being collected nowadays a determination not to accept them, founded upon a supposed impossibility. We must subordinate the teachings of the schools to those of well-observed facts, and, without choosing to throw ourselves with break-neck impetuosity into the course of a revolution, we must remain impartial in the investigation of the facts which will perhaps force us to accept it. I will only add that prudence commands us to be strict in their appreciation.

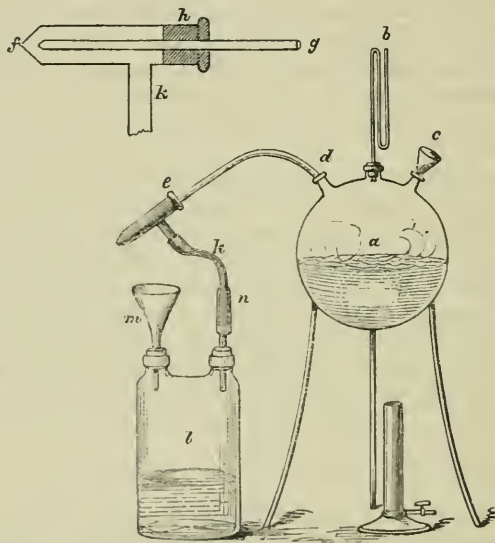
XLVI. *An Improvement in Filtration.* By Prof. JOHN C. DRAPER, M.D., *College of the City of New York*.\*

TO those who have been unable to employ Bunsen's improvement in filtration because of an insufficient supply of water to produce the necessary exhaustion, it may be of interest to know that all the practical advantages may be obtained by means of a simple and inexpensive apparatus, which it is the object of this communication to describe.

The apparatus consists of a stout boiler (*a*), with three openings accommodating respectively a pressure-gauge (*b*), a supply-funnel (*c*) for the introduction of water, and a tube (*d*) connecting with the exhausting arrangement or steam vacuum-tube (*e*), represented in section at *f*, in which *f* is a brass or glass tube  $\frac{1}{2}$  an inch in diameter, terminating in a small conical opening or nozzle  $\frac{1}{32}$  of an inch in diameter. In the interior of *f* and passing air-tight through the stuffing-box (*h*) there is a similar tube (*g*) about  $\frac{1}{8}$  inch in diameter, and terminating in a nozzle or opening like *f*, and about  $\frac{1}{64}$  of an inch in diameter. At *k* a tube  $\frac{1}{4}$  inch in diameter opens into *f*.

\* Communicated by the Author.

The nozzle-tubes *f*, *g* being placed in position as shown in the figure, steam is raised in the boiler (*a*); this passing through the tube *d e* to *g*, issues with violence from the nozzle; and in passing through the nozzle of *f* produces an exhaustion in the



interior of the tube *f* which may be applied as desired by a flexible tube attached to *k*. The vacuum produced depends partly upon the shape of the nozzles, and partly on their relative position. The latter adjustment is obtained by slipping the tube (*g*) through the stuffing-box (*h*) until the proper position is found. In the arrangement employed by me, I have without difficulty obtained with a pressure of one atmosphere of steam in the boiler an exhaustion capable of raising mercury eight inches perpendicularly in a tube attached to *k*, the exhaustion increasing steadily as the pressure of the steam increased.

At *l m* the filtering-apparatus is represented as attached to the steam vacuum-tube (*f*) by the flexible india-rubber connector (*n*).

New York, March 12, 1870.

XLVII. *Note on the Temperature and Heating-powers of Flames.*  
By W. M. WATTS, D.Sc.\*

THE April Number of the Philosophical Magazine contains a paper by Messrs. Silliman and Wurtz on the temperatures of flames, in which certain conclusions on some practical points of importance are stated.

The following numbers are correctly given as representing the amounts of heat evolved by the complete combustion of equal volumes of the following gases:—

Hydrogen . . . . .	34462
Carbonic oxide . . . . .	33612
Marsh-gas . . . . .	104504
Olefiant gas . . . . .	166012

But the conclusion that the heating-power of olefiant gas is greater than that of an equal volume of hydrogen is condemned as erroneous, and the authors give as the result of their investigations “that the powers of the flames of pure hydrogen and pure olefiant gas, even when used to the greatest advantage to heat water below its boiling-point, are almost or quite identical.”

There seems to be throughout the paper a confusion between the *heating-power* of a gas and the *temperature of its flame* in air.

It is quite true that, because the specific heat of steam, which is one of the products of combustion of olefiant gas, is greater than the specific heat of carbonic anhydride (the only product of combustion of carbonic oxide), the calculated flame-temperature of the latter gas is higher than that of the former; but, for the very same reason, if these gaseous products of combustion expend their heat in raising the temperature of water, the greatest effect will be produced by the olefiant gas, although the temperature of its flame is not so high as that of the flame of carbonic oxide. If the gases give up the whole of their heat to the water, it is obvious that the calorific effects must be precisely in the ratio of the numbers already quoted; and even if they escape at a temperature considerably above 0° C., their relative effects will be nearly the same. If we suppose the products of combustion to have a temperature of 200° C. when they escape, the following quantities of heat will still be available by the combustion of equal volumes of carbonic oxide and olefiant gas:—

Gas burnt.	Pounds of water raised from 0° C. to 100° C.
14 lbs. Carbonic oxide . . . . .	314
14 lbs. Olefiant gas . . . . .	1546

And these numbers are almost exactly in the same ratio as the

\* Communicated by the Author.



numbers 33642 and 166012. We conclude, therefore, that the numbers quoted on p. 292, in which, according to Messrs. Silliman and Wurtz, "several most grave errors are involved," do, in reality, strictly represent the heating-effects of the different gases when employed to the greatest advantage in heating water below 100° C.

The omission of the correction for the latent heat of steam in Bunsen's original calculation of the temperature of the hydrogen-flame has been already pointed out by Dibbits\*.

In the calculation of the temperature of the hydrogen-flame given in this paper (p. 294) there is also an error in assigning to liquid water the specific heat 0.4805 instead of 1: the formula should be

$$x = \frac{34462 - 9(637 - 48.05)}{4.3245} = 6743^{\circ} \text{ C.}$$

Another of the conclusions arrived at in this paper is erroneous, viz. "That, of all known gases, the highest calorific effects†, under ordinary atmospheric conditions, are obtainable from *carbonic oxide*, whose calorific value, above 100° C., is about 3000° C."

The flame of cyanogen is hotter than that of carbonic oxide—the calculated temperature of the flame of cyanogen in air being 3519° C., and in oxygen no less than 10557° C. It must be remembered, however, that these results of calculation in no way express the actual flame-temperatures. The calculated temperature of the oxyhydrogen-flame is 6743° C.; but the experiments of Deville‡ and Bunsen§ agree in fixing between 2500° C. and 2800° C. The following are the calculated and experimental flame-temperatures of certain gases. It will be seen that the flame of cyanogen is shown by experiment also to have the highest temperature:—

	Calculated.	Experimental.
Hydrogen in air . . .	2701° C.	2024° C.
Hydrogen in oxygen . .	6743	2844
Carbonic oxide in air . .	2996	1997
Carbonic oxide in oxygen.	7067	3033
Cyanogen in air . . .	3519	3297
Cyanogen in oxygen . .	10557	
Olefiant gas in air . . .	2727	
Olefiant gas in oxygen .	8606	

\* Pogg. *Ann.* vol. cxxii. p. 497.

† Read "flame-temperatures."

‡ *Leçons sur la Dissociation*, p. 281.

§ Pogg. *Ann.* vol. cxxxi. p. 161.

XLVIII. *Note on the Atomic Volume of Solid Substances.* By JAMES DEWAR, F.R.S.E., *Chemical Demonstrator in the University, Lecturer on Chemistry, Veterinary College, Edinburgh*.\*.

THE investigation of the volume retained by different elementary substances, when combined in the solid condition, has attracted the attention of many chemists. We have only to look at the laborious memoirs of Schröter, Kopp, Playfair and Joule, Boullay, Filhol, and others, to be convinced of the great amount of labour expended on the subject. Nor is it at all remarkable that so many workers should take to this field of research, when we remember the simplicity of the laws regulating the combining volumes of gaseous substances, and the probable extension of some such similar law to the solid condition of matter. Emboldened by analogy, the forementioned workers endeavoured to find some constant to which volumes of elements and compounds held the relation of some simple multiple, and thus extend the apparent simplicity of Prout's law of combining weights to combining volumes. The great object in view was evidently to extend the speculations and laws of Dalton and Gay-Lussac to the volumes of solid substances, and thus to arrive at some general explanation of the results. However creditable the desire to reveal simplicity out of the apparent chaos, no one, in examining the subject, can help arriving at the conclusion that the means employed to extract the seeming harmony from the results were purely arbitrary. It does not follow, however, that the results were fruitless, although no great generalization was discovered. The solid state of matter is relatively far more complicated than either the liquid or gaseous condition. The uniformity of expansion of gaseous matter, and the easy comparison of liquid substances under similar conditions, enable us to arrive at some satisfactory conclusions regarding the volume in these states: but in examining solid matter, we have no guarantee that the substances are under similar physical conditions; we cannot, therefore, expect the same uniformity in the results. But although, strictly speaking, we may entertain grave doubt on the real value of the results, yet in some cases we cannot help recognizing some curious analogies, especially on comparing similar classes of compounds. It is not the object of this note either to criticise or discuss the labours and speculations of others, no originality being claimed in the subject matter itself, all that is original being merely the addition of a few new analogies.

The first important discovery in the subject of atomic volumes was made by Schröter. He observed that the equivalent volume of oxygen, obtained by subtracting the volume of metal in the

\* From the Proceedings of the Royal Society of Edinburgh, Session 1869-70. Communicated by the Author.

free state from the volume of the oxide, gave, approximately, the same value of 5.2 in the oxides of copper, zinc, cadmium, lead, mercury, iron, cobalt, and titanium. In other words, the oxygen occupied the same volume in each combination. Other classes of oxides gave a volume of twice or half the above number. In order to arrive at the volume of the oxygen, Schröter started with the premises that the metal in the combined state occupied the same volume as the uncombined metal. Granting, for the present, that oxygen has a definite volume in combination in the oxides, it is clear that the volume obtained by difference will vary with the volume of the combined metal. The same method applied to the oxides of the less dense metals would give a negative volume to the oxygen; and in these cases we must admit condensation to have taken place in the metal itself. We may have three cases, therefore, according as the volume of the combined metal differs or not from that of the uncombined. If it remains the same in combination, we obtain the real volume; if it condenses, the volume is a minimum; if it expands, a maximum. Seeing that the oxygen in the dense metals has the volume 5.2, we may regard the greater or smaller volume obtained from some oxides as the result of condensation or expansion of the metal. Supposing the above volume (5.2) to exist generally in the oxides, we should have a condensation in the less dense metals in combination, approaching very nearly, in the case of potassium, sodium, and aluminium, to one-third, and in calcium, magnesium, and strontium to nearly one-half, of the volume in the free state. Thus far, then, this number would give a rough explanation in admitting condensation in many of the metals.

I have thought that it would be interesting to compare this volume with the volume of oxygen when it is combined with solid substances other than metallic, and to take a series of analogous combinations. For this purpose the chlorine family are well fitted in their respective combinations with potassium, and these with oxygen. The following Table contains the best known density determinations and volumes of chloride, bromide, and iodide of potassium, compared with the densities of chlorate, bromate, and iodate.

	Specific gravity.	Volume.	Difference.	Mean vol. of O (= 16).
K Cl .....	1.977	37.6 }	15	5
K ClO <sup>3</sup> .....	2.326	52.6 }		
K Br .....	2.69	44 }	7	2.3
K BrO <sup>3</sup> .....	3.271	51 }		
K I .....	3.0	55.3 }	-1.6	-0.5
K IO <sup>3</sup> .....	3.979	53.7 }		

The total volume of the oxygen in chlorate of potash, on the supposition that the chloride of potassium retains its original volume in combination, is 15; whereas it is only 7 in bromate of potash, if we allow that the bromide of potassium retains its original volume; and it appears to occupy no volume in iodate of potash, assuming that iodide of potassium maintains its original volume. The apparent disappearance of the volume of the oxygen, in changing iodide of potassium into iodate, is analogous to the apparent loss of volume of many salts in their water of hydration, the salt occupying the volume of the crystal water taken as ice, as pointed out many years ago by Playfair and Joule. It is clear that, in assuming that the halogen compounds of potassium retain their primitive volume in their oxidized derivatives, we place these compound substances in the same position as the metals in the simple oxides. Now, we saw that in many oxides the volume of the oxygen varied, and that, in all probability, from metallic condensation taking place during the act of combination. The metals having the lowest density and the greatest atomic volume condense the most in combining. Generally speaking, if we examine the volumes of the halogen salts in the above Table, it is clear that the equivalent volumes increase, chloride of potassium being 37, bromide 44, and iodide 55.3, and their relative stability diminishes. The equivalent volumes of chlorine, bromine, and iodine are identical in the liquid state; and thus the formation of the respective potassium compounds is one of the results of unequal condensation, the coefficient of contraction in the formation of chloride of potassium being 0.46, bromide 0.29, iodide 0.23 per unit volume. Their formation is attended with the evolution of very different amounts of heat. The following Table contains some of the constants found with reference to combination and solution.

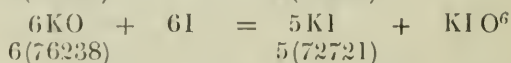
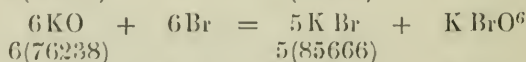
Constants of Group.

	Contraction per unit volume.	Total heat.	Heat of solution.	Diffusion times (relative).	Coefficient of expansion per equivalent volume	Specific heat per atom.
K Cl.....	0.46	97086	3874	74.5	0.001429	12.88
K Br.....	0.29	85666	4522	119	0.001848	13.47
K I .....	0.23	72721	4847	166	0.002358	13.60

Generally speaking, the number found for bromide of potassium is nearly the mean of those attached to chloride and iodide. A similar observation has recently been made by M. Valsen in examining the equivalent capillary constants of these bodies.



Looking at the atomic thermal number, there is a far greater likelihood of condensation taking place in the bromide and iodide of potassium in the combined state than in the case of the chloride, seeing that it would be relatively far more difficult to condense. But neither the chlorate, bromate, nor iodate can be produced through the direct addition of oxygen to the respective halogen salt. And the chlorate, it is well known, evolves heat on giving off its oxygen, and thus necessitates an absorption of heat during combination. It is just possible that the heat produced during the decomposition is the result of the necessary expansion of volume in the chloride of potassium in combining with oxygen, and its return to its normal volume on losing it. It makes no change in volume to suppose that, in the one case, the oxygen is added as a whole to the chloride of potassium, or, in the other, that it is between the potassium and chlorine, each occupying its individual volume unchanged; but it would alter greatly the heat evolved in so doing. If oxygen combined with chloride of potassium as a whole, without any condensation taking place, the natural result would be an evolution of heat. But if the addition of the oxygen diminishes the coefficient of contraction, as compared with that of the free compound, then we have a physical explanation of the evolution of heat on decomposition. In this case the actual work performed by the condensation of oxygen is retained in a potential form, and therefore reappears as heat on its decomposition. If, now, we examine the mode in which the oxygen is attached to the respective halogen compounds, we can trace, as a necessary consequence, the retention of varying amounts of energy. Chlorate, bromate, and iodate of potash are formed by a similar chemical reaction, according to the following formula of exchange, given in equivalents, the whole reaction supposed to take place in the presence of water:—



We have appended the thermal equivalents attending the formation of these bodies in a large volume of water. It will be obvious on comparing the formation of chlorate of potash, through the above reaction, that it may be the result of absorption of heat; whereas it is certain that the formation of iodate of potash must be attended with an evolution of heat, or else

cold must be the result of their action. In special experiments, made with the object of determining the thermal action, neither absorption nor evolution of heat could be detected. Thus the formation of iodate of potash is attended with an evolution of heat. This would then accord with the easy transformation of the chlorates into iodates, or of chloric acid into iodic acid, and the easy transformation of the iodide of potassium into the iodate, through the action of permanganate of potash, seeing that we must have an evolution of heat. The oxygen, therefore, may be assumed to be in a very different condition relatively to the other elements; or else we must suppose that it has not affected the coefficient of contraction, certainly not to have diminished it. The author throws out this simply as a possible explanation; he is well aware that many other explanations might be given, all possibly equally satisfactory. But a physical explanation, however far it may lie from the truth, seems to convey to us the clearest ideas of what may possibly take place.

There is one point connected with the subject of volumes that requires very careful attention. All bodies in combining do not unite with condensation; that is, the volume of the compound might exceed the volumes of the isolated constituents; and yet a large evolution of heat might take place during its formation. A well-known example is that of iodide of silver. Now M. Fizeau has shown that iodide of silver contracts regularly with increase of temperature, and M. Ste.-Claire Deville has given an explanation of this anomaly. Deville believes that bodies combine at such a temperature as would be required to transform the volume of the compound to that of the sum of the volumes of its constituents in the free state. Applying this to iodide of silver, it is clear that contraction must take place, and in all similar cases where we have an increase of volume. One cannot help associating this increase of volume with a purely physical change of state, such as the change of water with expansion into ice. Now, as Sir William Thomson has proven that pressure lowers the freezing-point of water, and Mousson has actually liquefied ice by enormous pressure, if the formation of a chemical compound is analogous to a physical change of state, we ought to be able by mere pressure to decompose a chemical compound, if the formation of that compound is attended with an increase of volume. No doubt, in order to get experimental proof of this fact, we must use a relatively weak chemical compound, one attended with the evolution of no great amount of heat; and the well-known experiments of Joule on the effect of pressure on amalgams seems to confirm this anticipation. Joule has shown that the amalgams of zinc, lead, and tin are decomposed by pressure alone; and these are the amalgams produced with the least con-

traction of any. In order to get definite proof of the expansion, it is, of course, necessary to use the specific gravity of mercury in the solid state. Now, Joule states, as the mean of his experiments, that mercury in the solid form has the specific gravity 15.19, whereas in the above amalgams it would have the density of only 14.1. The observations of Matthiessen on the specific gravity of alloys enables us to confirm Joule's results:—

Lead Series (A. Matthiessen).

	Specific gravity.	Calculated specific gravity.	$\frac{V+V'}{v}$
Pb <sup>2</sup> Hg .....	11.979	12.008	1.0024
Pb Hg.....	12.484	12.358	0.9899
Pb Hg <sup>2</sup> .....	12.815	12.734	0.9937

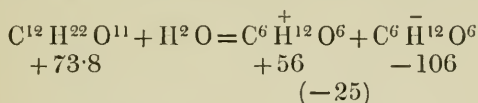
The specific gravity of the mercury used in calculating the mean density was 13.573. Now, seeing that there is little or no contraction, and even in one case a slight expansion, in taking the above specific gravity of mercury, the higher density of mercury given by Joule as the result of his experiments would necessarily lead to an expansion in their formation. To illustrate the effect of pressure on the composition of an amalgam, let us take Joule's experiments on the tin-amalgam. The composition of this amalgam was 100 of mercury to 51.01 of tin, and the specific gravity 10.518. The effect of 5400 lbs. pressure for thirty days changed the amalgam, so that it had ultimately the composition 100 of mercury to 384 of tin. It is natural to believe, therefore, that the effect of pressure in this case is quite analogous to the inverse change of state when a body that has expanded in changing its state has been subjected to its influence.

In the early part of this paper we saw that the volume of oxygen in some oxides, instead of being 5.2, was sometimes double this amount, or even more. It has also been remarked that if the metal in combining were to expand, the volume of the oxygen would appear as a maximum. This apparently large volume of the oxygen seems to belong to suboxides, such as suboxides of mercury and copper, and oxide of silver. If we suppose, now, that this large increase of volume in the oxygen is the result of an expansion in the metal in combining with the normal oxide, it is possible that mere pressure would decompose these oxides, at least in part, into metal and the higher oxide. The instability of a body of this type, such as suboxide of mercury, is well known, mere trituration effecting the liberation of metal with formation of the higher oxide. In this way, therefore, it seems to support the argument adduced.

XLIX. *Note on Inverted Sugar.* By JAMES DEWAR, F.R.S.E.,  
*Chemical Demonstrator in the University, Lecturer on Chemistry,*  
*Veterinary College, Edinburgh\*.*

FOR some time past an animated discussion has been going on in the columns of the *Comptes Rendus de l'Académie des Sciences* between MM. Dubranfaut and Maumené regarding the nature of inverted sugar. M. Dubranfaut, many years ago, made many valuable additions to our knowledge concerning the composition and reactions of various sugars, especially in explaining the result of the action of dilute acids on cane-sugar. He explained the lævo-rotatory action of inverted sugar, and its rapidly varying power with the temperature, as the result of a molecule of water in reacting with a molecule of cane-sugar, generating one molecule of glucose and one of lævulose. Dubranfaut believed that inverted sugar consisted of a mixture of glucose and lævulose in equal weights; and although he did not make a direct analysis of the product, yet he was justly entitled to assume that it was so constituted, seeing that generally it agreed with a mean of the properties of inulin sugar and dextrose.

In order to support the above view, he separated lævo-glucose from the inverted sugar, through the insolubility of the lime compound, and compared its properties with pure lævulose. The decomposition would, according to Dubranfaut, be as follows:—



So thoroughly had his facts and explanations been accepted by chemists generally, that, up to a recent date, no one discovered any flaw in his researches, and therefore no doubt was thrown on the validity of thist heory. Recently Maumené has reinvestigated the composition of inverted sugar by analysis. He has attempted to separate the two sugars through the action of chloride of sodium. The dextro-glucose forms a well-defined crystalline compound with chloride of sodium, whereas the lævulose does not form any compound. The results obtained by this method differ greatly from theory. Instead of finding 50 per cent. of lævulose, he found 88 per cent. In repeating the experiments of Dubranfaut on the separation of lævo-glucose by hydrate of lime, he has not met with any better results; in fact his results are quite opposed to those of Dubranfaut.

Apart altogether from expressing an opinion on the merits of

\* From the Proceedings of the Royal Society of Edinburgh, Session 1869-70. Communicated by the Author.



the views entertained by the different parties to this discussion, the author has thought some observations on the same subject might not be unworthy of notice at the present time.

Linneman, many years ago, applied the process of hydrogenation to the sugars that had been found so successful in treating the simpler organic substances. In this way he obtained mannite from inverted sugar, the following reaction taking place:—



Mannite had long been known to be the product of certain kinds of fermentation, and to occur as a secondary product in the vinous fermentation; but it was this elegant synthesis of Linneman that first clearly showed the connexion. But although inverted sugar can be changed into mannite, the next point that demands a solution is, supposing the inverted sugar to be composed of equal quantities of dextrose and lævulose, are they both transformed by hydrogenation into mannite? or is only one of them, and which? Linneman seems to have directed his attention to the solution of this question. He states that it is only the lævulose that is so affected. The reason why he entertains the above view is not given. In all likelihood he thought that, just as Berthelot had changed mannite by a peculiar fermentation into lævo-glucose, so would the lævo-glucose in inverted sugar be hydrogenized into mannite.

In repeating the action of sodium-amalgam on inverted sugar, I have not seen any reason why the one sugar any more than the other should be supposed to generate the mannite. The following is a description of the mode by which the sugar was inverted and hydrogenized:—20 grms. of cane-sugar were dissolved in 150 grms. of water, and inverted through the action of 2 grms. of sulphuric acid, keeping the solution at the temperature of 70° C., afterwards adding pure carbonate of barium, filtering, and then adding 12 grms. of sodium in the form of a weak amalgam. The action took place without any evolution of hydrogen. If the amalgam was impure, from the presence of other metals, it evolved hydrogen at once, and the solution became brown; otherwise it remained perfectly clear. After one month the solution gave no trace of sugar with the alkaline copper solution. It was then carefully neutralized with dilute sulphuric acid, evaporated on the water-bath, the greater part of the sulphate of sodium separated by crystallization, and the residue treated with boiling 70-per-cent. alcohol, the solution filtered and allowed to crystallize. Sometimes the mannite did not crystallize until all the alcohol had evaporated, leaving a syrup that slowly assumed the crystalline form. The product had no rotatory power. In no case was the sugar entirely changed into mannite; a gummy

substance was invariably left, that would not crystallize after exposure to the air for months. Mannitan, or some similar body, may be one of the products.

Dextro-glucose made from honey gave mannite when treated in the same way, having exactly the same melting point as ordinary mannite. In treating milk-sugar with dilute sulphuric acid, changing into gallactose and hydrogenizing, dulcite was not isolated; but I have not specially studied the reaction.

L. *On the Theory of the Variation of Temperature in Gases in consequence of Changes in their Density and Pressure.* By the Rev. J. M. HEATH\*.

IF  $v$  is the volume of a unit of weight of a gas,  $p$  its pressure, and  $t$  its temperature, then the equation of Mariotte and Gay-Lussac may be thus written:— $pv = k(1 + \alpha t)$ , or  $= A + Bt$ , and its differential  $d(pv) = Bdt$ . From these equations we learn that every change in the temperature of the gas must cause, or be caused by, a corresponding change in the magnitude  $pv$  which depends upon  $p$  and  $v$  only.

Now  $d(pv) = p dv + v dp$ , which is therefore equal to  $Bdt$ . But the theory which has been propounded for the last twenty-five years, and is all but universally received as established beyond all doubt, is that one of these two terms only, viz.  $p dv$ , is alone connected, whether as cause or as effect, with variation in the temperature. It is held that whenever the gas, being under the pressure  $p$ , is expanded or contracted by the quantity  $dv$ , there is a certain loss or gain in the quantity of heat, and a corresponding variation, therefore, in the temperature.

This doctrine is reconcilable with the older one only so long as the causes which they respectively assign for the same effect, viz. a variation of temperature, are the same—that is, so long as  $p dv = d(pv)$ . But  $d(pv) = p dv + v dp$ . This identity can therefore only exist so long as  $v dp = 0$ , or so long as  $p$  is a constant.

This fact, that the modern doctrine is true as long as  $p$  is invariable, is perhaps the explanation of the facility with which it has been almost universally admitted. For the experiment by which the value of the mechanical equivalent of heat was supposed to be determined from the expansion of gas by heat, was made upon a gas kept under a constant pressure. An invariable weight  $P$  was raised by the additions made to the heat in a gas; but a portion of the heat so added, proportional to the product of  $P$  into the height through which it had been raised, did not appear to have contributed at all to the altera-

\* Communicated by the Author.

tion of temperature, which was that which was due solely to the remainder of the heat after this portion had been separated from it.

It appears, therefore, that a conclusion, legitimately deduced from a certain experiment, has been too hastily generalized, and is now enunciated as true in all cases, beyond the limitations which, though not observed, were nevertheless present at, and controlled the results of the original experiment. It is true that as often as gas is condensed or rarefied *under an invariable pressure*, there will be a change of temperature representing a gain or loss of heat proportionate to  $p dv$ . But when the pressure is not constant, the variation of temperature will be proportional to  $d(pv)$ , and *not* to  $p dv$ .  $d(pv)$  may be nothing;  $p dv$  may be  $= -v dp$ . In that case there is no variation of temperature; and in all cases where  $d(pv)$  has any value different from nothing and different from  $p dv$ , there will be a change of temperature which does *not* represent a quantity of heat proportional to  $p dv$ .

In a short paper upon this subject, published in the April Number of this Magazine, I attempted to represent the same argument (adopting the language and conceptions of the new philosophy itself) as I am now urging in the older and more generally familiar modes of expression. I believe that  $p v$  is the true expression for the maximum internal potential energy,  $v dp$  is the elementary potential energy expended, and  $p dv$  is the elementary work done. So long as  $p dv + v dp = 0$ , the internal forces, or potential energy, do *all* the internal work that is done, and no other, and  $d(pv)$  or  $B dt = 0$ , and there is no change of temperature. When  $p dv + v dp$  has a value, positive or negative, *i. e.* when  $p dv$  is  $>$  or  $<$   $v dp$ , then, and then only, is the temperature affected.

Milland, Liphook, April 18, 1870.

## LI. The Province of Mineral Chemistry.

By HERMANN KOLBE\*.

ORGANIC chemistry, once the neglected sister of the maturer inorganic chemistry, has become so honoured and courted since Liebig introduced her as a young science into the chemical world, that her relative has gradually been thrown into the shade. But now we are beginning to see that inorganic chemistry is not deserving of this neglect, but that in her lap she hides a store of undiscovered treasure—the promised reward of those who cherish her.

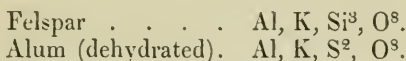
\* From the *Journal für Praktische Chemie*, neue Folge, vol. i. p. 1. Translated by Dr. T. E. Thorpe, Owens College.

The lively interest excited by the discovery of isomorphism is now succeeded by an ardent zeal in searching out the origin of the numberless instances of isomerism in organic compounds. Hitherto isomeric compounds have scarcely been observed in inorganic chemistry, much less in mineral chemistry. This may be owing partly to the simpler constitution of inorganic compounds, in consequence of which isomerism is not possible to the same extent as in organic chemistry, and partly to the fact that until now we have altogether neglected to study inorganic substances with respect to their chemical constitution.

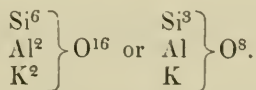
The truth of the latter assertion becomes at once evident when we attempt to frame a reply to any inquiry respecting the constitution of the naturally occurring silicates—felspar for example—such as what are the proximate constituents of these compounds, and what are their several functions. Let us see the extent of our knowledge in this direction.

Berzelius regarded felspar as built up according to the formula  $\text{Al}^2 \text{O}^3 3\text{SiO}^3 + \text{KO SiO}^3$  (old atomic weights), viewing it as a double salt of neutral silicate of alumina and silicate of potash, analogous to dehydrated alum.

Such a comparison is no longer applicable, since as the atomic weight of oxygen is now doubled, and silicon is regarded as a tetratomic element, the molecule of felspar contains three atoms of silicon, whilst that of alum possesses only two of sulphur.



Afterwards, when Gerhardt\* declared that all investigations designed to elucidate chemical constitution could only end in disappointment, thinking that he had rendered such investigations superfluous by his theory of types (that is, by the mere mechanical use of a soulless schematism), felspar became water in which the atoms of hydrogen were substituted by silicon, aluminium, and potassium, and its constitution was expressed by the formula



These expressions possess nothing beyond the signification of a sum in addition over the purely empirical formulæ  $\text{Si}^6 \text{Al}^2 \text{K}^2 \text{O}^{16}$ , or  $\text{Si}^3 \text{Al K O}^8$ ; that is to say, they simply indicate that the sum of the affinities of the atoms of silicon, aluminium, and potassium present in felspar is equivalent to that of 32 or of 16 atoms, as

\* *Comptes Rendus des travaux de Chimie*, par Laurent et Gerhardt. 1851, p. 65.



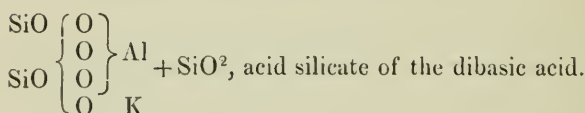
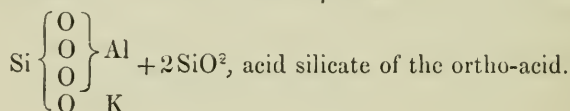
the case may be, of hydrogen. Nothing whatever is signified with respect to the proximate constituents of felspar.

But how may we consider the elements in felspar to be united to proximate constituents? and how again may these be supposed to be combined among themselves? The cases possible are numerous.

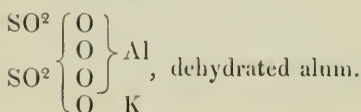
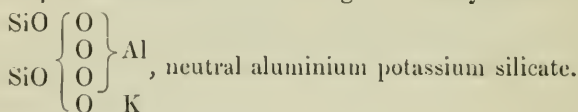
Felspar may contain the tetrabasic orthosilicic acid corresponding to orthocarbonic acid, of which the hydrate is consti-

tuted according to the formula  $\text{Si} \left\{ \begin{smallmatrix} \text{O} \\ \text{O} \\ \text{O} \\ \text{O} \end{smallmatrix} \right\} \begin{smallmatrix} \text{H} \\ \text{H} \\ \text{H} \\ \text{H} \end{smallmatrix}$ . Or it may contain

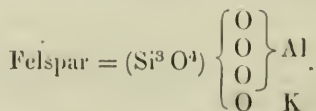
the dibasic silicic acid  $(\text{SiO})'' \left\{ \begin{smallmatrix} \text{O} \\ \text{O} \end{smallmatrix} \right\} \begin{smallmatrix} \text{H} \\ \text{H} \end{smallmatrix}$ , corresponding to ordinary carbonic acid. In either case felspar would be an acid silicate:



The neutral silicate corresponding to the latter compound would possess a constitution analogous to dehydrated alum.



But felspar may be represented as a neutral salt when we assume in it the presence of a tetrabasic silicic acidradical,  $(\text{Si}^3 \text{O}^4)$ ,



Whether the supposition of this tetrabasic silicic acid, which in some degree corresponds to tetrabasic pyrophosphoric acid,  $(\text{P}^2 \text{O}^3) \text{O}^4 \text{H}^4$ , is permissible cannot here be discussed.

There is still another method of representing felspar as a neu-

tral salt, when we assume therein a similar method of combination to proximate constituents as that which is supposed to occur in the case of potassium dichromate.

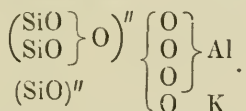
Thus, when two atoms of the divalent radical of chromic acid,  $\text{CrO}^2$ , are so combined with an atom of oxygen that its affinities are divided between them, we obtain the divalent acid radical  $\left(\begin{smallmatrix} \text{CrO}^2 \\ \text{CrO}^2 \end{smallmatrix} \right\} \text{O}''$  of the potassium dichromate,  $\left(\begin{smallmatrix} \text{CrO}^2 \\ \text{CrO}^2 \end{smallmatrix} \right\} \text{O}'' \cdot \text{K} \cdot \text{O} \cdot \text{K}$ .

Now, if we assume that two atoms of the divalent silicic acid radical  $\text{SiO}$  are united in a similar manner to an atom of oxygen, we have a new dibasic silicic acid, of which the neutral potash-salt is built up according to the formula  $\left(\begin{smallmatrix} \text{SiO} \\ \text{SiO} \end{smallmatrix} \right\} \text{O}'' \cdot \text{K} \cdot \text{O} \cdot \text{K}^*$ . If

we further suppose this salt to be combined with the neutral potash-salt of normal dibasic silicic acid, as indicated by the formula

mula  $\left(\begin{smallmatrix} \text{SiO} \\ \text{SiO} \end{smallmatrix} \right\} \text{O}'' \left\{ \begin{smallmatrix} \text{O} \\ \text{O} \\ \text{O} \end{smallmatrix} \right\} \begin{smallmatrix} \text{K} \\ \text{K} \\ \text{K} \end{smallmatrix}$ , and substitute an atom of trivalent

aluminium for three atoms of potassium, we obtain felspar possessing the following constitution:—



Now to which of these four isomeric compounds, of which the number might doubtless be increased, felspar belongs, or if indeed the rational composition of this mineral may properly be expressed by such formulæ, is a question which cannot be discussed, since up to the present time the necessary experimental foundation is entirely wanting.

May we not expect that fresh experimental investigations designed with this object will furnish such data, and that in the future we may possess opinions on the constitution of the silicates as well founded as those which we now hold on the constitution of the compound ethers and their components?

I am not of the opinion of those who deem it impossible to reap in the fields of mineral chemistry a scientific harvest, worthy of the name, by any experimental investigations beyond those of mere quantitative analysis. When we commence investigating

\* Titanite,  $\left(\begin{smallmatrix} \text{SiO} \\ \text{SiO} \end{smallmatrix} \right\} \text{O}'' \cdot \text{O} \cdot \text{Ca} + \left(\begin{smallmatrix} \text{TiO} \\ \text{TiO} \end{smallmatrix} \right\} \text{O}'' \cdot \text{O} \cdot \text{Ca}$ , may be regarded as a double calcium-salt of this silicic acid and of the corresponding titanic acid, or as a simple neutral salt containing a mixed acid radical,  $\left(\begin{smallmatrix} \text{SiO} \\ \text{TiO} \end{smallmatrix} \right\} \text{O}'' \cdot \text{O} \cdot \text{Ca}$ .

the silicates and the compounds of the mineral kingdom generally, in a manner similar to that by which we have sought to discover in organic compounds the proximate constituents and their functions (that is, by a careful study of their decompositions, substitutions, and syntheses), then we may certainly expect soon to gather results as splendid as those which organic chemistry has so lavishly presented to us.

The future province of the mineralogical chemist is, therefore, not simply to analyze minerals and to determine their empirical composition, and then from this empirical composition empirically to construct rational formulæ, but to create new methods of investigation, and from their results to deduce arguments to elucidate the chemical constitution of such minerals.

LII. *On the Course of Geodesic Lines on the Earth's Surface.*

*By* CAPTAIN A. R. CLARKE, R.E., F.R.S.\*

THE geodesic line has always held a more important place in the science of geodesy among the mathematicians of France, Germany, and Russia than has been assigned to it in the operations of the English and Indian triangulations. Here, indeed, it has been completely set aside, partly because the long arcs measured are in the direction of the meridian (itself a geodesic line), and partly because the angles of a geodesic triangle cannot be actually observed. If we consider three points on a spheroid and suppose them joined by geodesic lines, then the angles of this triangle will differ slightly from the angles which would be measured by a theodolite; in other words, the angle in which geodesics proceeding from A and B intersect in C differs from the angle contained by the two planes which, containing the normal at C, pass through A and B. But the difference of length between the plane curve distances CA, CB and the corresponding geodesic distances, we can show to be immeasurably small for any such distance as two or three degrees. It may also be proved that the calculation of spheroidal triangles as spherical is only correct when the observed angles have been reduced to the geodesic angles. Still the difference is so very small for such triangles as are formed by mutually visible points on the earth's surface that it has been generally disregarded. But it would be somewhat hasty in the advancing state of science to conclude that geodesic lines have no necessary place in geodesy. Both the extreme precision now attained in the measures of base lines and angles, and the vast extents of country over which triangulations are being carried, make the consideration of even the smallest refinements not superfluous. If pro-

\* Communicated by the Author.

gress is to be made in the investigation of the figure of the earth, certainly no "small quantities" may be set aside without at least a distinct idea of their magnitude and ultimate effect. For instance, the *course* of the geodesic line joining two points on a spheroid of small excentricity is probably not investigated in any English work, and it is therefore not unnecessary to inquire how far this line departs from any of the plane curves joining its extremities.

Although the observed angles of a triangulation are not geodesic angles, yet in the calculation of the distance and reciprocal bearings of two points far apart and connected by a long chain of triangles we may fall upon the geodesic line in this way:—If  $A, Z$  be the points, then to start the calculation from  $A$  we get by some preliminary calculation the approximate azimuth of  $Z$  at  $A$ , or the angle made by the direction of  $Z$  with either of the sides  $AB, AC$  of the first triangle. Let  $P_1$  be the point where this line or direction intersects  $BC$ ; then to find  $P_2$ , where the line cuts the next triangle-side  $CD$ , we make the angle  $BP_1P_2$  such that  $BP_1P_2 + BP_1A = 180^\circ$ . This fixes  $P_2$ ; and  $P_3$  is fixed by a repetition of the same process; so for  $P_4, P_5, \dots$  Now it is clear that the points  $P_1, P_2, P_3, \dots$  so computed are those which would be actually fixed by an observer with a theodolite, proceeding in the following manner. Having set the instrument up at  $A$  and turned the telescope in the direction of the computed bearing, an assistant places a mark  $P_1$  on the line  $BC$ , adjusting it until it is bisected by the cross hairs of the telescope fixed at  $A$ . The theodolite is then removed from  $A$ , placed over the mark  $P_1$ , and the telescope turned to  $A$ ; the horizontal circle, or merely the telescope itself on its axis, is then turned through  $180^\circ$  and the instrument clamped. The assistant then places a mark  $P_2$  on the line  $CD$  so as to be bisected by the cross hairs of the telescope, which is then removed to  $P_2$ ; and in the same manner is  $P_3$  fixed. Now it is clear that the string of points  $P$  approximates to the geodesic line; for the plane of any two consecutive elements  $P_{n-1}P_n, P_nP_{n+1}$  contains the normal at the common point  $P_n$ .

With the exception of the greater part of Norway, Sweden, and Turkey, the whole of Europe may be considered to be covered with chains of triangulation binding its countries together. From the north-western extremity of the Hebrides to Dunkirk in France there is a strong connexion by the British triangulation; this line may be continued through France to Berne, and by a triangulation, possibly not quite complete, through Switzerland and Italy to Corfu and Palermo. Again, a line may be drawn from Cadiz to St. Petersburg: if the necessary triangulation is not quite complete, it will doubtless be so before many years



have passed. The grand chain of triangles upon the great European arc of parallel between Valentia and Orsk runs along the parallel, and does not approach a great circle or geodesic course.

## 1.

It would be difficult to make the subject we are going to consider intelligible without going to the root of the matter. Therefore, at the risk of being tedious, we propose to investigate, in the briefest possible manner, the leading characteristics of the geodesic curve. If we suppose the position of a point on an ellipsoid of revolution to be determined by its distance  $\zeta$ , measured from one of the poles along the curve of a meridian, and by its longitude  $\omega$ , then,  $r$  being the distance of the point from the axis of revolution, the length of a curve traced on the surface is

$$s = \int (r^2 d\omega^2 + d\zeta^2)^{\frac{1}{2}}.$$

If this length is to be a minimum between the given extremities, we shall most quickly arrive at the distinctive character of the curve by giving a variation  $\delta\omega$ , a function of  $\zeta$ , to  $\omega$ . Thus

$$\begin{aligned} \delta s &= \int \frac{r^2 d\omega}{ds} d\delta\omega \\ &= \delta\omega \cdot \frac{r^2 d\omega}{ds} - \int \delta\omega \cdot d\left(\frac{r^2 d\omega}{ds}\right); \end{aligned}$$

and consequently for the minimum,

$$\frac{r^2 d\omega}{ds} = C.$$

To fix our ideas, let longitude be measured positively from west to east, and azimuths from north through east, back to north. Let  $\alpha$  be the azimuth of the element  $ds$  of the curve, then

$$ds \cos \alpha = -d\zeta,$$

$$ds \sin \alpha = r d\omega;$$

and the latter of these, substituted in the characteristic of minimum, gives

$$r \sin \alpha = C.$$

Now, if  $u$  be what is sometimes called in treatises on the conic sections the eccentric angle, or the reduced latitude,  $r = a \cos u$ ,  $a$  being the major semi-axes of the spheroid; and if  $u_1 \alpha_1$  be the values of  $u \alpha$  at the initial point  $A$ ,

$$\cos u_1 \sin \alpha_1 = \cos u \sin \alpha.$$

The relation here expressed is that which exists between two sides,

$90^\circ - u_p$ ,  $90^\circ - u$ , of a spherical triangle, and the angles opposite to them,  $180^\circ - \alpha$  and  $180^\circ - \alpha_p$ . Let the third side of this triangle be  $\sigma$ , and the third angle  $\varpi$ , then

$$d\sigma \cos \alpha = du,$$

$$d\sigma \sin \alpha = \cos u \, d\varpi.$$

If  $\phi$  be the latitude of a point P on the geodesic, so that  $a \tan u = b \tan \phi$ , then

$$\sin u = \sin \phi (1 - e^2 \cos^2 u)^{\frac{1}{2}}.$$

Also

$$d\zeta \sin \phi = dr = -a \sin u \, du,$$

from which we get

$$\left. \begin{aligned} ds &= a(1 - e^2 \cos^2 u)^{\frac{1}{2}} d\sigma, \\ d\omega &= (1 - e^2 \cos^2 u)^{\frac{1}{2}} d\varpi. \end{aligned} \right\} \quad . \quad . \quad . \quad (1)$$

This completely determines the auxiliary spherical triangle, and through it the latitude and longitude of any point P at a distance  $s$  from A measured along a geodesic which has a given initial azimuth. With respect to the angle  $\varpi$ , if we neglect, as we propose to do henceforth,  $e^4$  and higher powers of  $e^2$ ,

$$\omega = \varpi - \frac{e^2}{2} \int \cos^2 u \, d\varpi;$$

but by the consideration of the spherical triangle we see that

$$d\varpi = d\sigma \frac{\sin \alpha}{\cos u} = d\sigma \frac{\sin \alpha_l \cos u_l}{\cos^2 u};$$

$$\therefore \omega = \varpi - \frac{e^2}{2} \sigma \sin \alpha_l \cos u_l. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

## 2.

Now let P B be two points on the geodesic,  $s \, s'$  their distances from A,  $\omega \, \omega'$  their longitudes,  $u \, u'$  their reduced latitudes, then

$$\sin u = \sin u_l \cos \sigma + \cos u_l \sin \sigma \cos \alpha_p,$$

$$\sin u' = \sin u_l \cos \sigma' + \cos u_l \sin \sigma' \cos \alpha_l;$$

from these, by means of (1), we can express  $s$  and  $s'$  in terms of  $\sigma$  and  $\sigma'$ , or, inversely, express  $\sigma$  and  $\sigma'$  in terms of  $s$  and  $s'$ . Then if  $\varpi, \varpi'$  correspond to  $\omega, \omega'$ ,

$$\left. \begin{aligned} \varpi &= \omega + \frac{e^2}{2} \sigma \sin \alpha_l \cos u_l, \\ \varpi' &= \omega' + \frac{e^2}{2} \sigma' \sin \alpha_l \cos u_l. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (3)$$

By spherical trigonometry we have these two equations :

$$\cot \alpha_l \sin \varpi = \cos u_l \tan u - \sin u_l \cos \varpi,$$

$$\cot \alpha_l \sin \varpi' = \cos u_l \tan u' - \sin u_l \cos \varpi',$$

which by the elimination of  $\alpha_l$  give

$$\sin \varpi' \tan u = \sin \varpi \tan u' + \tan u_l \sin (\varpi' - \varpi). \quad (4)$$

Expressing next  $\varpi$  and  $\varpi'$  in terms of  $\omega$  and  $\omega'$ , and writing for brevity  $\omega_l, \varpi_l, \sigma_l$  for  $\omega' - \omega, \varpi' - \varpi, \sigma' - \sigma$ ,

$$\sin \varpi = \sin \omega + \frac{e^2}{2} \sigma \sin \alpha_l \cos u_l \cos \omega,$$

$$\sin \varpi' = \sin \omega' + \frac{e^2}{2} \sigma' \sin \alpha_l \cos u_l \cos \omega',$$

$$\sin \varpi_l = \sin \omega_l + \frac{e^2}{2} \sigma_l \sin \alpha_l \cos u_l \cos \omega_l.$$

The next step is to substitute these expressions in (4). Putting  $\alpha'$  for the azimuth of the geodesic at B, and making use of the equations

$$\sin \sigma \cos \alpha_l = \cos u_l \sin u' - \sin u_l \cos u' \cos \varpi,$$

$$-\sin \sigma \cos \alpha' = \cos u' \sin u_l - \sin u' \cos u_l \cos \varpi,$$

and effecting other obvious simplifications, we get

$$\sin \omega' \tan u = \sin \omega \tan u' + \sin \omega_l \tan u_l$$

$$+ \frac{e^2}{2} \{ \sigma \cos \alpha_l \sin \omega_l - \sigma_l \cos \alpha \sin \omega \}.$$

But since we are to neglect  $e^4$ , we may put within the parenthesis,

$$\left. \begin{aligned} \sin \omega_l &= \sin \omega' \frac{\cos u_l \sin \sigma_l}{\cos u \sin \sigma'}, \\ \sin \omega &= \sin \omega' \frac{\cos u' \sin \sigma}{\cos u \sin \sigma'}. \end{aligned} \right\} \quad (5)$$

Also, for brevity, put

$$\tan u_0 = \tan u' \frac{\sin \omega}{\sin \omega'} + \tan u_l \frac{\sin \omega_l}{\sin \omega'};$$

and since  $\tan u - \tan u_0 = (u - u_0) \sec^2 u$ , we get, finally,

$$u = u_0 + \frac{1}{2} e^2 \cos u \left( \frac{\sigma \sin \sigma_l}{\sin \sigma'} \cos u_l \cos \sigma - \frac{\sigma_l \sin \sigma}{\sin \sigma'} \cos u' \cos \alpha' \right). \quad (6)$$

This determines the latitude of any point P on the geodesic joining AB, the longitude of P being  $\omega$ .

3.

We now leave this part of the subject, and turn to the consideration of the plane which, containing the normal at A, passes through B. Let Q be any point in the curve of intersection of this plane with the surface of the spheroid,  $\omega$  the longitude of Q, U its reduced latitude, then

$$\sin U - \cos U \tan u_0 = e^2 \left\{ \sin U - \sin u_l - \frac{\cos U \sin \omega}{\cos u' \sin \omega'} (\sin u' - \sin u_l) \right\}.$$

We shall not stop to prove this equation (which is exact), but merely indicate that it is most readily obtained by getting the equation of a plane containing the normal at A and making a given angle with the meridian at that point. In this equation, substitute first the coordinates of P expressed in terms of  $u$ ,  $\omega$ , then those of B in terms of  $u'$ ,  $\omega'$ , and between the two equations so obtained eliminate the azimuth angle. The result is the equation just written down. Since in the right-hand member we may neglect  $e^2$  among the terms within the parenthesis, then by the use of (5) and the following, which is easily proved by a spherical triangle,

$$\sin \sigma' \sin U = \sin \sigma \sin u' + \sin \sigma_l \sin u_p,$$

we get

$$\sin U - \cos U \tan u_0 = e^2 \sin u_l \left( \frac{\sin \sigma + \sin \sigma_l - \sin \sigma'}{\sin \sigma'} \right),$$

or

$$U = u_0 + \frac{e^2}{2} \frac{4 \sin \frac{1}{2} \sigma \sin \frac{1}{2} \sigma_l}{\cos \frac{1}{2} \sigma'} \cos u_0 \sin u_p. \quad (7)$$

It may be remarked that in this equation  $\cos u_0$  may be replaced by  $\cos u$ , since by (6) the difference is of the order  $e^2$ . We may apply this equation (7) to the plane which, containing the normal at B, passes through A. If Q' be a point in the curve of intersection, having the same longitude or on the same meridian with Q, and if U' be its reduced latitude,

$$U' = u_0 + \frac{e^2}{2} \cdot \frac{4 \sin \frac{1}{2} \sigma \sin \frac{1}{2} \sigma_l}{\cos \frac{1}{2} \sigma'} \cos u_0 \sin u'. \quad (8)$$

4.

We now know from (6), (7), (8) the relative positions in latitude of the three points P, Q, Q', which are on any the same meridian, P being on the geodesic, and Q Q' on the two plane



curves. Thus, the difference of latitude of  $Q'$  north of  $Q$  is

$$\frac{e^2}{2} \frac{4 \sin \frac{1}{2} \sigma \sin \frac{1}{2} \sigma_l}{\cos \frac{1}{2} \sigma'} \cos u (\sin u' - \sin u_l); \quad . \quad . \quad (9)$$

and if  $a\delta u$  be the distance of  $P$  north of  $Q$ ,

$$\delta u = \frac{e^2}{2} \cos u \left\{ \frac{\sigma \sin \sigma_l}{\sin \sigma'} \cos u_l \cos \alpha_l - \frac{\sigma_l \sin \sigma}{\sin \sigma'} \cos u' \cos \alpha' \right. \\ \left. - \frac{4 \sin \frac{1}{2} \sigma \sin \frac{1}{2} \sigma_l}{\cos \frac{1}{2} \sigma'} \sin u_l \right\}. \quad . \quad . \quad (10)$$

This expression completely determines the course of the geodesic; and it is to be remarked that  $\sigma$  has not been supposed small. We may alter the form of (10) by eliminating  $\alpha'$  through the equation

$$-\cos u' \cos \alpha' = \sin u_l \sin \sigma' - \cos u_l \cos \sigma' \cos \alpha_l.$$

Thus we get

$$\left. \begin{aligned} \delta u &= \frac{e^2}{2} \cos u \{ H \cos u_l \cos \alpha_l + K \sin u_l \}, \\ H &= \frac{\sigma \sin \sigma_l - \sigma_l \sin \sigma \cos \sigma'}{\sin \sigma'}, \\ K &= \sigma_l \sin \sigma - 4 \frac{\sin \frac{1}{2} \sigma \sin \frac{1}{2} \sigma_l}{\cos \frac{1}{2} \sigma'}. \end{aligned} \right\} . \quad . \quad (11)$$

If we desire to trace the geodesic line, not as passing through two given points, but as starting from a point  $A$  in a given azimuth, we may refer its different points to the corresponding points of the curve of intersection of the vertical plane at  $A$  which touches the geodesic at that point. In order to do this, we must put in (11)  $\sigma_l = \sigma' - \sigma$ , and in the result put  $\sigma' = 0$ , making the point  $B$  move up to  $A$  along the geodesic. The result of this operation is

$$\left. \begin{aligned} \delta u &= \frac{e^2}{2} \cos u \{ H \cos u_l \cos \alpha_l + K \sin u_l \}, \\ H &= \sigma \cos \sigma - \sin \sigma, \\ K &= -\sigma \sin \sigma + 4 \sin^2 \frac{1}{2} \sigma. \end{aligned} \right\} . \quad (12)$$

We may apply this equation to verify an expression given by Bessel in the *Astronomische Nachrichten*, No. 3, for the difference between the astronomical azimuth of a point  $B$  at  $A$  and the azimuth of the geodesic  $AB$  at  $A$ . The difference is evidently

$$\frac{du \sin \alpha}{\sin \sigma};$$

and the azimuth of the vertical plane exceeds that of the geodesic by

$$\left. \begin{aligned} \delta\alpha &= \frac{e^2}{2} \cos u_1 \sin \alpha_1 \{ H \cos u_1 \cos \alpha_1 + K \sin u_1 \}, \\ H &= 1 - \frac{\sigma}{\tan \sigma}, \\ K &= \sigma - 2 \tan \frac{1}{2} \sigma. \end{aligned} \right\} . \quad (13)$$

5.

It is interesting to consider the case of geodesic lines starting from a point on a spheroid of small excentricity and diverging in all directions. First, to confine our attention to a single line, it is well known, and may be readily inferred from the auxiliary spherical triangle, that a geodesic touches alternately two parallels equidistant from the poles—the difference of longitude between the successive points of contact being constant and something less than  $180^\circ$ , depending on the angle at which it cuts the equator. Now suppose a line starting from (for simplicity) a point on the equator with the azimuth  $\alpha$ ; the osculating plane at that point cuts the equator again at the opposite point N. As a point P moves along the geodesic towards N, the angle  $\varpi$  of the auxiliary spherical triangle increases from 0; and when it becomes  $\pi$ , then  $\sigma$  also becomes  $=\pi$ , and P has reached the equator, its longitude being, by (2),  $\pi - \frac{1}{2}e^2\pi \sin \alpha$ . Since in (12) H is negative for all values of  $\sigma$  from 0 to  $\pi$ , the geodesic lies wholly on the south side of the osculating plane at the initial point; and its distance south, when  $\sigma=\pi$  and P is on the equator, is  $\frac{1}{2}e^2 \cos \alpha \pi$ . We infer from this that all geodesics proceeding from the same point on the equator have an approximately equal length  $\frac{1}{2}\pi e^2$  (about 36' in the case of the earth) intercepted between the meridian through N and the equator. Consequently the ultimate intersections of the geodesics will form an envelope like the evolute of an ellipse, or the hypocycloid  $x^{\frac{2}{3}} + y^{\frac{2}{3}} = k^{\frac{2}{3}}$ , N being the centre of the curve. (Mr. Cayley informs me that this property of geodesics is referred to in some remarks in Jacobi's *Vorlesungen über Dynamik*, Berlin, 1866, which, however, I have not seen. It is noticed that if geodesic lines, starting from a given point, intersect so as to form an envelope, then each line is a shortest line only up to its point of contact with the envelope and no further.) If the lines diverge from a point not on the equator, the diameter of the envelope will vary as  $\cos^2 u$ .

6.

We shall now apply the results we have obtained to the geodesic line joining Cadiz and St. Petersburg. Referring it to

the curve of intersection formed by the plane containing the normal at Cadiz, we shall compute the meridional distances of points P on the geodesic from corresponding points Q on the plane curve. We take approximate latitudes and longitudes for the terminal points.

Cadiz.		St. Petersburg.	
Latitude	. 36° 32'	. 59° 56'	
Longitude	. 6 18 W.	. 30 17 E.	

From these we obtain, neglecting  $e^2$  in the calculation,

$$\begin{aligned}\alpha_1 &= 33^\circ 13' \\ \alpha' &= 61^\circ 29' \\ \sigma' &= 33^\circ 1'\end{aligned}$$

Taking for  $a$  the equatorial radius of the earth 20,926,060 feet, and for the ratio of the axes 294 : 295, we have

$$\log a \frac{e^2}{2} = 4.85013.$$

It is convenient to divide the distance  $\sigma'$  into eight equal parts, and so determine seven points on the geodesic. Since  $\sigma'$  is about  $33^\circ 1' 28''$ , we take for the eighth part  $4^\circ 7' 40''$ . The result of the calculation is contained in the following Table:—

$\sigma$ .	Q.		P north of Q.
	Latitude.	Longitude.	
0 0 0	36 32	6 18 W.	ft. 0.0
4 7 40	39 57	3 21 "	255.0
8 15 20	43 17	0 6 "	458.0
12 23 0	46 31	3 32 E.	589.2
16 30 40	49 37	7 37 "	637.0
20 38 20	52 34	12 13 "	597.3
24 46 0	55 18	17 28 "	473.2
28 53 40	57 47	23 28 "	271.4
33 1 20	59 56	30 17 "	0.0

It appears from this that, at the middle point of the geodesic, the length of meridian intercepted between it and the plane curve is upwards of 600 feet. With respect to the azimuths of the geodesic at its extremities, they differ, as we find by equation (13), from the azimuths of the vertical planes by  $20''.37$  at Cadiz and  $12''.71$  at St. Petersburg. How such quantities should be dealt with in practical geodesy is a question foreign to the object of this investigation, which is merely to ascertain what sort of magnitudes we have to deal with in the divergence of the geodesic from plane curves.

7.

If in the expression for  $\delta u$  we make  $\sigma = \sigma_1 = \frac{1}{2} \sigma'$ , we get for  $\delta u$  at the middle point of the line,

$$\delta u = 2e^2 \cos u \frac{\sin^2 \frac{1}{4} \sigma}{\cos \frac{1}{2} \sigma} \left\{ \frac{\frac{1}{4} \sigma}{\tan \sigma} \sin u - \sin u_1 \right\}, \quad (14)$$

where  $u$  is the latitude of the middle point. We may apply this to another example. Take the chain of triangles in India extending from the Kurrachee base in the west to the Calcutta base in the east. The approximate latitudes of these positions, taken from a common map of India, are  $25^\circ 0'$  and  $22^\circ 30'$  respectively, and the difference of longitude  $21^\circ 10'$ . From these we obtain

$$\begin{aligned} \alpha_1 &= 92^\circ 58' \\ \alpha' &= 101^\circ 34' \\ \sigma &= 19^\circ 40' \\ u &= 24^\circ 7' \end{aligned}$$

and referring the geodesic to the curve formed by the vertical plane at Calcutta, we get at the middle point

$$a\delta u = 46.6 \text{ feet.}$$

Also the differences of the azimuths of the geodesic from those of the vertical planes are—at Kurrachee  $2''.04$ , and at Calcutta  $3''.76$ . The smallness of these quantities is the consequence of the line being nearly perpendicular to the meridian.

8.

If we suppose the length of the line small, so that powers of  $\sigma'$  above the fourth may be neglected, (11) takes the form

$$\begin{aligned} \delta u = \frac{e^2}{2} \sigma \sigma_1 \cos u \left\{ \frac{\sigma' + \sigma}{3\sigma'} (\sin u' - \sin u_1) \right. \\ \left. + \frac{1}{12} (\sigma^2 + 3\sigma\sigma_1 + \sigma_1^2) \sin u_1 \right\}, \quad (15) \end{aligned}$$

by which the course of any geodesic of a few degrees in length may be traced. If we may neglect  $\sigma^4$ , then comparing (15) with (9) we find

$$\frac{QP}{QQ'} = \frac{\sigma' + \sigma}{3\sigma'} = \frac{1}{2} + \frac{\sigma - \sigma_1}{6\sigma'},$$

showing that the geodesic lies between the plane curves.

In the case of a geodesic joining two points on the same parallel,

$$\delta u = \frac{e^2}{48} \sigma \sigma_1 (\sigma^2 + 3\sigma\sigma_1 + \sigma_1^2) \sin 2u.$$



If S be the point in which this geodesic cuts the meridian which is equidistant from A and B, S' another point on this same meridian such that a vertical plane at S' passes through both A and B, and S'' the intersection with this meridian of the plane containing the normals at A and B, then, neglecting  $\sigma^5$ ,

$$S''S = \frac{5}{6} S'S'.$$

The consideration of (15) or of (13) also makes it evident that, under some circumstances or within certain limits of azimuth, the geodesic joining AB will intersect one of the plane curves between those points.

## 9.

Bessel, in one of his papers in the *Astronomische Nachrichten*, gives, but without demonstration, an expression for the difference in length between the geodesic and one of the plane curves. In case of the accident of a misprint, it is worth while to verify it. Q being a point on the (southern) plane curve at a distance  $a\delta u$  south of P on the geodesic, if we draw an arc of parallel through P meeting the plane curve in the point R east of P, we have  $PR = a\delta u \tan \alpha$ ; the difference of longitude of R and P is therefore

$$\delta\omega = \delta u \tan \alpha \sec u; \quad . \quad . \quad . \quad . \quad (16)$$

and we may suppose all the points of the plane curve to be referred in this manner to the geodesic. Now, returning to the expression for the length of a curve on the surface: when  $\omega$  is increased by  $\delta\omega$ ,

$$\frac{ds}{d\zeta} = \left( r^2 \frac{d\omega^2}{d\zeta^2} + 1 \right)$$

is increased by

$$r^2 \frac{d\omega}{ds} \left( \frac{d\delta\omega}{d\zeta} \right) + \frac{1}{2} \left( \frac{d\delta\omega}{d\zeta} \right)^2 r^2 \frac{d\zeta^3}{ds^3},$$

the first term of which, when integrated, is, by reason of the character of the geodesic, zero. Hence the increment in length in passing from the geodesic to the plane curve is, since  $ds \cos \alpha = -d\zeta$ ,

$$\delta s = -\frac{1}{2} a^2 \int \cos^2 u \cos^3 \alpha \left( \frac{d\delta\omega}{d\zeta} \right) d\zeta.$$

As we require merely the first or principal term in the value of  $\delta s$  and none else, we may put  $d\zeta = -a du$ , and

$$\delta s = \frac{1}{2} a \cos^2 u, \cos^3 \alpha, \int \left( \frac{d\delta\omega}{du} \right)^2 du.$$

Now from (15) (16), since  $\sin u' - \sin u_1 = \sigma' \cos \alpha_1 \cos u_1$ ,

$$\begin{aligned}\delta\omega &= \frac{1}{6}e^2\sigma(\sigma'^2 - \sigma^2) \sin \alpha_1 \cos u_1, \\ \left(\frac{d\delta\omega}{du}\right)^2 du &= \left(\frac{d\delta\omega}{d\sigma}\right)^2 \frac{d\sigma}{du} d\sigma \\ &= \frac{e^4}{36} \frac{\sin^2 \alpha}{\cos \alpha} \cos^2 u (\sigma'^2 - 3\sigma^2)^2 d\sigma; \\ \therefore \delta s &= \frac{ae^4}{288} \cos^4 u \sin^2 2\alpha \int (\sigma'^2 - 3\sigma^2)^2 d\sigma \\ &= \frac{ae^4}{360} \sigma^5 \cos^4 u \sin^2 2\alpha.\end{aligned}$$

This will give an approximation to the truth when  $\sigma$  is not very large. The coefficient  $\frac{1}{360}ae^4$  is only 2.66 feet; and if  $\sigma$  were, for instance,  $10^\circ$ , the maximum value of  $\delta s$  would be less than a hundredth of an inch. The difference between the lengths of the two plane curves is of a higher order.

Southampton, April 16, 1870.

LIII. Goodricke's *Theory of Algol*. By T. S. ALDIS, M.A.,  
Late Scholar of Trinity College, Cambridge\*.

THERE are some consequences of this theory which I have not seen noticed. The comparative light of Algol when full and when eclipsed enables us to compute the relative size of Algol and its satellite. The light varies from 25 to 9, about; and the disks are therefore as 25 to 16, and the diameters as 5 to 4. Knowing the time of eclipsing, we can easily compute the distance, which on this scale is 27. Hence, knowing the periodic time, the mean density is found to be one-fifth that of the sun. This result is probable, as Algol is unquestionably brighter than our sun, which would be the case naturally if Algol be less cooled down or condensed. The gigantic size of the satellite is thus easily explicable. Both it and Algol must undergo much condensation before they arrive at the condition of our system.

The constant diminution of the period also naturally results from the hypothesis. With such a low density for Algol, and such a large satellite, the density of the latter must be very low. It is indeed opaque, as is proved by spectral analysis, but probably in quite a gaseous state. It will therefore be very susceptible to the influence of a resisting medium. This can be no part of Algol's system; for every part of that would gain, by friction on the interior parts, velocity, till it revolved freely round

\* Communicated by the Author.

Algol; so such a medium would not retard the satellite, but move with it. The medium can be none other than the interstellar medium, through which the stars pass from point to point without impressing on any part any sensible rotation. It follows naturally, then, from Goodricke's theory, that the period should be diminished, as the light medium must produce some effect on so rare and rapid a body.

Yet the diminution of period is very small. It amounts to about one second in twelve years at most. This gives in one revolution a diminution of about  $\frac{1}{360000000}$  of the whole. Now for Encke's comet there is a diminution of  $\frac{1}{11000}$  of the period in one revolution. Of course the circumstances are very different; but allowance being roughly made for them, the comet appears to suffer most. This is probably due partly to the less density of the comet, partly to the fact that the light medium is not the only resister of the comet. Whatever interplanetary media there may be rotating round the sun, the comet in its perihelion, moving far faster than they, will be hindered, whilst the contrary action at aphelion would be far from sufficient to restore the balance. Tidal lagging, too, or, without lagging, the contraction of Algol and its attendant's spheroidal forms as they cool, would tend to diminish the mean motion, and so prevent its increase being as great as it otherwise would be. It thus appears probable that the diminution of Algol's period may be accounted for by Goodricke's hypothesis. This diminution will continue till, in the process of condensation, tidal lagging comes into greater play, when it may be slowly reversed.

The above calculations are rude and approximate. It would take more time than the question is worth, even if we had sufficient data, to discuss the problem thoroughly. I shall be glad if this paper be found to advance a step in our knowledge of the heavens.

*Note.*—In the paper "On the Nebular Hypothesis," October 1869, it may be noticed that the elongation of form, which theory suggests for the attended planets as their condition on severance from the central mass, is probably none other than the crescent form, like a fragment of a ring partially embracing the central body, which is by no means without example in the various nebulae.

Manchester Free Grammar School,  
April 14, 1870.

LIV. *On the Spectra of some Gases under High Pressures.*

By M. A. WÜLLNER\*.

A PAPER by M. Wüllner has already appeared in this Journal† on the various shapes which the spectrum of a gas traversed by an electric spark is capable of assuming when the pressure of the gas or the temperature produced by the passage of the spark is varied. In his first research M. Wüllner had only worked with hydrogen, oxygen, and nitrogen under low pressures; but he has subsequently resumed this investigation with the aid of M. Bettendorff; and the use of a far more powerful induction-coil has enabled him to examine how the spectrum of the electrical discharge is affected when, instead of a rarefied gas, it traverses a gas under a more or less high pressure.

*Apparatus.*

The apparatus used was arranged so that the pressure inside the spectrum-tube could be easily varied from an almost perfect vacuum up to two or three atmospheres. For this purpose it consisted essentially of a large U-tube with legs of unequal length. The longer was at least 2·5 metres long and was open at the top; it contained the mercurial column which measured and applied the pressure. In the bend of the U-tube was a glass cock by which the mercury of the tube could be allowed to flow out. The Geissler's tubes used had essentially the same construction as those previously used; they were provided with two adjutages, each of which had a glass stopcock.

The shorter leg of the U-tube extended about 770 millims. from the bend; and to it was fused one end of the Geissler's tube. The lower adjutage was connected, through a Liebig's apparatus containing concentrated sulphuric acid and a tube containing anhydrous phosphoric acid, with the gasometer. The upper adjutage was connected with a Geissler's air-pump provided with drying-arrangements containing phosphoric acid. The spectrum-tube was of the ordinary shape—that is to say, a very short capillary tube connecting two larger ones. It had two pairs of electrodes, one with a distance of 8 centims.; the others ended just at the extremities of the capillary part of the tube, and were about 16 millims. from each other.

In making the experiment, it was first of all necessary to fill the apparatus with perfectly pure and dry gas. After having introduced into the U-tube a quantity of mercury enough

\* Translated from an abstract in the *Bibliothèque Universelle de Genève* for September 1869, of the original in Poggendorff's *Annalen*, vol. cxxxvii. p. 337.

† Phil. Mag. S. 4. vol. xxxvii. p. 405.



to balance the atmospheric pressure, some well-dried gas was passed in; the tube was then exhausted, and this process repeated until an almost perfect vacuum was obtained; the tube was then left for twenty-four hours in communication with the drying-apparatus of the pump. The experiment could then be proceeded with by allowing perfectly pure and dry gas to enter while the electrical current passed between the two most distant electrodes. When the pressure in the apparatus had become equal to the atmospheric pressure, it was increased by gradually pouring mercury into the long leg of the U-tube, which was provided with a scale to measure the variations in pressure.

#### a. *Hydrogen.*

At low pressures, the use of a large Ruhmkorff's inductorium with six Grove's elements gave successively three spectra identical with those described in M. Wüllner's first memoir. First, when the pressure is the lowest that can be attained with a Geissler's pump, a discontinuous spectrum, or one of the second class, is obtained, consisting of six groups of very brilliant green lines; then, from about 1 to 2 or 3 millims., a spectrum consisting of three bright lines,  $H\alpha$ ,  $H\beta$ ,  $H\gamma$  (the spectrum of the second class described by Plücker); and lastly, with a further increase of pressure, a continuous (or first-class) spectrum extending from the orange to the blue, with a certain number of bright lines beyond  $H\alpha$  and  $H\beta$  (which still remain). From M. Wüllner's new observations, this spectrum remains in its full brightness at 200 millims. pressure; thence it becomes paler, but otherwise retains the same appearance up to 400 millims. and even beyond. At 671 millims. the space between  $H\alpha$  and the commencement of the green is almost completely dark; the remainder of the continuous spectrum is still pretty bright; the line  $H\beta$  has almost disappeared, and in its place there is only seen a large band a little brighter than the surrounding parts of the spectrum.

As the pressure still increases, the spectrum again becomes more luminous, the yellow and orange parts gradually reappear, the line  $H\alpha$  is still very bright, but a little indistinct at the edges; beyond this line a continuous spectrum extends from the orange to the violet, and presents a maximum lustre in the place previously occupied by  $H\beta$ . The spectrum retains the same aspect up to 1400 millims., but becomes far more luminous; it is particularly brilliant about  $H\beta$ , which appears to have simply widened; the same is the case in the violet, where a broad bright band seems to have taken the place of the old line  $H\gamma$ .

As the pressure increases, the spectrum of hydrogen appears more and more like the absolutely continuous one of an incandescent solid body. At 2240 millims. (that is to say, nearly

three atmospheres) it almost exactly resembles a solar spectrum restricted to Fraunhofer's lines C and G\*. It merely differs by the presence of the line  $H\alpha$ , which, however, is much widened, and by the maxima of luminous intensity which it presents in the places where were the lines  $H\beta$  and  $H\gamma$ .

M. Wüllner concludes, from what has been said, that above a certain density the temperature of the jet, and with it the luminous intensity of the spectrum, increases continuously; so that we are warranted in assuming that, by subjecting the gas to a still higher pressure, or by raising the temperature of the jet, we should obtain a continuous spectrum without any bright line. He endeavoured to effect this by introducing a Leyden jar into the induced circuit, so that it was discharged between the two electrodes nearest to one another. With the Leyden jar, and in proportion as the pressure is increased, the spectrum of hydrogen is seen to pass through almost the same phases as when a powerful Ruhmkorff's inductorium is used; but the successive transformations of the spectrum are more rapidly effected. Thus at 300 millims. with the Leyden jar, the same appearance is presented as under three atmospheres with the induction-coil alone. At 560 millims. the spectrum is almost continuous.  $H\alpha$  is no longer a bright line, but a broad red band; and  $H\beta$  is no longer recognized, as before, by a maximum of luminous intensity. At 1000 millims. the spectrum becomes still more equalized; there is at most a maximum lustre where formerly the line  $H\alpha$  appeared; the temperature of the jet is so raised that the sodium-line appears distinctly as a bright line. Lastly, at 1230 millims. the lustre of the jet is so great that the sodium-line is reversed, and appears just like Fraunhofer's line D in the solar spectrum. Thus it is seen that an incandescent gas may produce a reversed spectrum, provided its temperature is high enough, and that it is not necessary for the principal source of light to be an incandescent solid.

The author pushed the experiment as far as a pressure of 1320 millims., at which the discharge of the Leyden jar became discontinuous. The appearance of the spectrum at this pressure was still the same, as was also its length. From the commencement of the experiment, at low pressures a spectrum was obtained restricted to the lines  $H\alpha$  and  $H\gamma$ ; and its limits remained constant during the whole of the experiment. It is not likely that the spectrum of hydrogen would ever extend further, even at the highest pressures; we have here even a good criterion that

\* With this limitation, however—that the spectrum of hydrogen at high pressures is a direct spectrum with no dark line, while the solar spectrum is reversed.

the continuous spectrum just described really belongs to hydrogen, and not to incandescent solid particles carried by the current.

b. *Oxygen.*

At low pressures M. Wüllner obtained with a large Ruhmkorff's coil and his six Grove's elements results quite agreeing with those previously obtained. At 10 millims. he observed a spectrum of the second class identical with that which Plücker had described; but beyond that, and in proportion as he increased the pressure, this spectrum lost more and more of its lustre. At 200 millims. scarcely more than a small portion of the lines composing it were perceived, those contained in the green and the blue. From this point the luminous intensity again increases, Plücker's spectrum-lines gradually reappear, and at the same time there is seen behind them a continuous spectrum, which is first seen in the green and then gradually spreads out, especially on the side of the more refrangible rays. At 650 millims. the continuous spectrum extends from the ray called  $O\alpha$  by Plücker to the middle of the interval between Fraunhofer's lines G and H. It is particularly bright in the green part, where its lustre almost causes the bright lines of the spectrum of the second class to disappear. Yet these, contrary to what was observed in the case of hydrogen, do not lose the definiteness of their contour, nor become broader, but are less well distinguished on a ground which is almost as brilliant as they. With Ruhmkorff's inductorium M. Wüllner could never with oxygen exceed a pressure of 800 millims. The continuous spectrum remained almost the same, with a rather more equal lustre in its different parts; and the bright lines always stood out with perfect precision on this luminous ground. Here also, though the lustre increased, the spectrum never extended beyond the limits within which it was contained at low pressures.

The use of a Leyden jar gave in this case the same results as the simple induced current, but with a far higher degree of intensity. At 30 millims., with a Leyden jar we have a very bright continuous spectrum quite resembling that which the induced current gave at 600 millims. As the pressure increases, the lustre of the continuous spectrum and of the bright lines increase at the same rate, especially in the orange and yellow parts. There, as in the case of hydrogen, the bright lines lose the precision of their contour and seem to spread out. At 280 millims. this part of the spectrum forms a continuous field, while everywhere else the bright lines continue, and stand out on a basis a little less bright than they. The phenomenon remains constant up to a pressure of 540 millims., which M. Wüllner never exceeded

in this experiment. The limits of the spectrum having remained in this case also the same throughout the whole experiment, we have a proof that the continuous spectrum on which the bright lines stand out really belongs to oxygen.

This gas, then, is distinguished from hydrogen by the fact that it is in the less refrangible part that the bright lines of its fundamental spectrum spread out and disappear and only leave a continuous spectrum, while in the case of hydrogen these lines first disappear in the more refrangible part. Moreover it never gave, like hydrogen, an absolutely continuous spectrum without any bright line. Even at still higher pressures than those we have indicated, and with a larger Leyden jar charged by a Holtz's machine, the bright lines remain perfectly distinct, at least in the more refrangible part of the spectrum, starting from the green.

### c. Nitrogen.

To investigate the spectrum of nitrogen, the author did not work as before on air, but on pure nitrogen; for it was more than probable that at high pressures oxygen would exert a disturbing action upon his experiments which it could not exert in very rarefied air.

At low pressures he obtained, as before, a beautiful continuous and fluted spectrum of the first class. At 25 millims. the flutings had already disappeared, except in the green and the blue. As the pressure increases, the less refrangible part of the spectrum becomes more and more obscured until a pressure of 260 millims. has been attained; there then scarcely remains more of the first-class spectrum of nitrogen than the blue and violet parts, which are still fluted, and already there is visible in the green part a bright line belonging to the spectrum of the second class. There are then gradually seen to appear an increasing number of these bright lines; and towards 500 millims. there is, as it were, a struggle between the two nitrogen-spectra. It is easy to see that they are quite distinct, and that the bright lines of the second have no connexion with the flutings of the first. When this latter has quite disappeared, a certain number of bright lines remain which stand out upon a continuous and illuminated ground. The bright lines first appear in the more refrangible part, and only last of all in the red. From 600 millims., and especially towards 760 millims., this spectrum is complete and very bright: this pressure could never be exceeded with the large Ruhmkorff's coil.

It is easily seen that the continuous spectrum, serving in this case also as a basis for the bright lines, forms an integral part of the second-class spectrum of nitrogen, by the fact that it stops at exactly the same limits as the bright lines. The author does



not admit that it can be confounded with the first-class spectrum of this gas, which always retains its flutings to the moment in which it quite disappears.

The Leyden jar gave exactly the same spectrum with more brightness in the colours.

From this abstract of M. Wüllner's memoir it will be seen that the spectrum of one and the same gas rendered incandescent by the passage of the electric spark varies materially with the temperature and pressure. The spectrum of hydrogen offers four very distinct appearances—that is to say, the spectrum with six groups of green lines, Plücker's spectrum with three bright lines, the continuous spectrum with two of these three lines ( $H\alpha$  and  $H\beta$ ), and finally the absolutely continuous spectrum produced under high pressures. Oxygen gives also four different spectra; that is to say:—(1) at the lowest limits of pressure, a second-class spectrum consisting of five groups of bright lines in the green and the blue; (2) at 1 millim. and below, a continuous spectrum consisting of broad bands, especially in the green and the blue; (3) about 10 millims., the second-class spectrum described by Plücker; and (4) finally, at high pressures, a continuous spectrum with a large number of bright lines persisting in the more refrangible part. Nitrogen gives two spectra—that is to say, a continuous but fluted spectrum at low pressures, and at high ones a continuous spectrum without fluting and on which are seen to appear a great number of bright lines, first in the green and the blue, and then also in the red. A more profound investigation will show whether these spectra are absolutely distinct. M. Wüllner is led to believe so, and therefore also to assume that the emissive power of incandescent gases for rays of different colours undergoes sudden variations as the pressure of the gases themselves gradually varies. This important point is not adequately cleared up by M. Wüllner's numerous observations.

On the other hand, M. Wüllner has been led by this latter research to modify the ideas which he at first shared with MM. Plücker and Hittorff, and according to which continuous or first-class spectra corresponded to a lower temperature than spectra with bright lines. It would seem, on the contrary, that continuous spectra correspond to the highest temperatures and pressures, which would be in conformity with Dr. Frankland's observations on the spectrum of the hydrogen-flame under high pressures. This, however, is a difficult point to verify; for it has been seen above that the lustre and the temperature of the electric jet do not vary in a regular manner with the pressure of the gas it traverses. M. Wüllner intends to resume these researches.

LV. *Description of a New portable Mercurial Barometer.*

By J. A. CALANTARIENTS.

*To the Editors of the Philosophical Magazine and Journal.*

19 Guildford Street, Russell Square, W.C.,  
April 20, 1870.

GENTLEMEN,

I BEG to enclose two drawings, with a description, of a new portable mercurial barometer which I have invented, and hope you will do me the favour of inserting it in the next Number of your Magazine.

I beg to enclose also the copy of a letter from Dr. Balfour Stewart, of Kew Observatory, addressed to the makers, together with the account of the comparison mentioned in that letter.

The slight discrepancy between the indications of my barometer and that of the standard is entirely owing to the carelessness of the makers, or rather of the man who tested and made the scale; for there is no reason whatever, as you will see from the principle on which it is constructed, why it should not be made as accurate as a standard one. The present scale, however, is correct.

Besides fulfilling the ordinary requirements of a barometer, it is, I believe, well adapted for taking mountain-measurements, as it is light, of small size, and compact form. Instead of mercury other fluids can, of course, be made use of, if thought preferable. I very much regret that professional duties have so long hindered me from taking the necessary steps for its publication. I invented it whilst a student of medicine at University College.

I remain,

Your obedient Servant,

J. A. CALANTARIENTS.

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Kew Observatory, Richmond, Surrey, S.W.,  
January 14, 1869.

GENTLEMEN,—I enclose an account of our comparison of Mr. Calantarients's barometer with our standard. The result is a fair one, and the instrument seems to be well compensated.

Yours truly,

Messrs. Newcombe and Co.

B. STEWART.

*Comparison of Calantarients's Barometer with the Standard Barometer of the Kew Observatory.*

inches.

Mean correction at	{	30 = -0.19	} temperature remaining constant.
		29 = -0.45	
		28 = -0.51	
		27 = -0.40	
		26 = -0.36	

Temperature.

$$\text{Mean correction at } \left\{ \begin{array}{l} 52 = -0.16 \\ 64 = -0.05 \\ 74 = -0.00 \\ 84 = +0.04 \\ 94 = +0.07 \\ 104 = +0.08 \end{array} \right\} \begin{array}{l} \text{pressure remaining} \\ \text{constant.} \end{array}$$

---

*Description of the Barometer.*

This instrument, as represented in figs. 1 and 2, consists of three parts:—

1st, of a glass tube (fig. 2, A B C) of small diameter, bent at C and ending in the closed bulb D ;

2ndly, of a tube (E F G) of larger diameter, joined to the first at E A and open to the atmosphere at G ;

3rdly, of a cylinder (H K L) joined to the first two at H and closed at L.

These three freely communicate with one another at E A H. The black spaces ending at B F K indicate the parts filled with mercury, the remainder of the tube A B C and the bulb D contain air slightly rarefied, and the remaining space in the cylinder K L is filled with liquid sulphuric ether.

From this arrangement it is evident that any variation in the atmospheric pressure would be communicated to the column of mercury F A B through the open tube G F, and the rise or fall of the point B indicate the exact degree of such variation. This, therefore, would form a barometer.

Now with regard to the effects of temperature. The cylinder H K L has been made in such proportion to the bulb D and the tube above it, that the ether and mercury contained in the first have the exact expansive power necessary to counteract that of air in the latter. For instance, suppose the temperature were to rise, the expansion of air in the tube and bulb B C D would tend to lower the point B ; but as the ether and mercury in the cylinder also expand at the same time and drive mercury into the tube E F G, the weight of this additional column would tend to raise the point B ; and as these two forces are equal and opposite in direction, the point B would remain stationary. If the proportion between the two is not exactly equal, it is easily restored, either by exchanging mercury for ether, or the contrary, as the case may require, in the cylinder H K L, or by varying the quantity of air in the bulb D.

The two contractions at the lower end of the cylinder are intended to prevent the ether escaping when the instrument is laid on its side.

The size of this barometer is somewhat smaller than twice that of the drawings ; but it can be made of any size, larger or smaller,

Fig. 1. Barometer in its case complete.

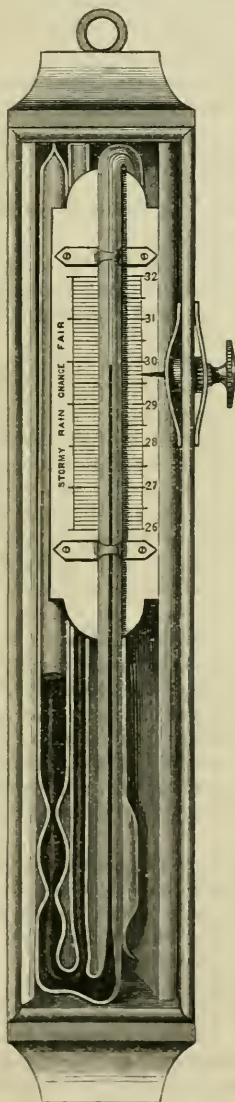
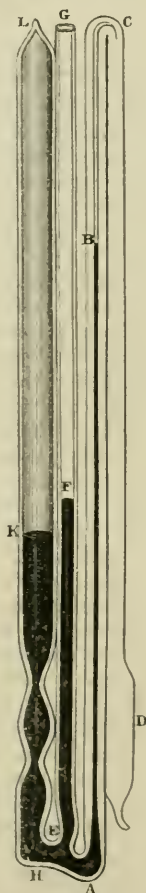


Fig. 2. The same as fig. 1, with the scale and index removed.





as required, and put together very compactly. The range of its indications can also be made longer by increasing the size of the bulb and the diameter of the tube EFG; but in the same proportion the cylinder would have to be made larger.

The chief novelty of this instrument consists in the means adopted for the correction of temperature; and this, I believe, can advantageously be employed for other purposes.

## LVI. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 312.]

Jan. 13, 1870.—Warren De La Rue, Vice-President, in the Chair.

THE following communications were read:—

“On Fluoride of Silver.—Part I.” By George Gore, F.R.S.

This communication treats of the formation, preparation, analysis, composition, common physical properties, and chemical behaviour of fluoride of silver.

The salt was prepared by treating pure silver carbonate with an excess of pure aqueous hydrofluoric acid in a platinum dish, and evaporating to dryness, with certain precautions. The salt thus obtained invariably contains a small amount of free metallic silver, and generally also traces of water and of hydrofluoric acid, unless special precautions mentioned are observed. It was analyzed by various methods: the best method of determining the amount of fluorine in it consisted in evaporating to dryness a mixture of a known weight of the salt dissolved in water, with a slight excess of pure and perfectly caustic lime in a platinum bottle, and gently igniting the residue at an incipient red heat until it ceased to lose weight. By taking proper care, the results obtained are accurate. The reaction in this method of analysis takes place according to the following equation,  $2\text{AgF} + \text{CaO} = \text{CaF}_2 + 2\text{Ag} + \text{O}$ . Sixteen parts of oxygen expelled thirty-eight equal parts of fluorine present. One of the methods employed for determining the amount of silver consisted in passing dry ammonia over the salt in a platinum boat and tube at a low red heat. The results obtained in the various analyses establish the fact that pure fluoride of silver consists of 19 parts of fluorine and 108 of silver.

Argentite fluoride is usually in the form of yellowish brown earthy fragments; but when rendered perfectly anhydrous by fusion, it is a black horny mass, with a superficial satin lustre, due to particles of free silver. It is extremely deliquescent and soluble in water; one part of the salt dissolves in 55 part by weight of water at  $15^{\circ}\cdot 5$  C.; it evolves heat in dissolving, and forms a strongly alkaline solution. It is nearly insoluble in absolute alcohol. The specific gravity of the earthy-brown salt is  $5\cdot 852$  at  $15^{\circ}\cdot 5$  C.; the specific gravity of its aqueous solution, at  $15^{\circ}\cdot 5$  C., saturated at that temperature, is  $2\cdot 61$ . By chilling the saturated solution, it exhibited the phenomenon of supersaturation, and suddenly solidified, with evolution of

heat, on immersing a platinum plate in it. The solution is capable of being crystallized, and yields crystals of a hydrated salt; the act of crystallization is attended by the singular phenomenon of the remainder of the salt separating in the anhydrous and apparently non-crystalline state, the hydrated salt taking to itself the whole of the water. The fused salt, after slow and undisturbed cooling, exhibits crystalline markings upon its surface.

The dry salt is not decomposed by sunlight; it melts below a visible red heat, and forms a highly lustrous, mobile, and jet-black liquid. It is not decomposed by a red heat alone; but in the state of semifusion, or of complete fusion, it is rapidly decomposed by the moisture of the air, with separation of metallic silver; dry air does not decompose it. In the fused state it slightly corrodes vessels of platinum, and much more freely those of silver.

The salt in a state of fusion, with platinum electrodes, conducts electricity very freely, apparently with the facility of a metal, and without visible evolution of gas or corrosion of the anode; a silver anode was rapidly dissolved by it, and one of *lignum-vitæ* charcoal was gradually corroded. A saturated aqueous solution of the salt conducted freely with electrolysis, crystals of silver being deposited upon the cathode, and a black crust of peroxide of silver upon the anode; no gas was evolved: with *dilute* solutions gas was evolved from the anode. By electrolysis of anhydrous hydrofluoric acid with silver electrodes, the anode was rapidly corroded.

The electrical order of substances in the fused salt was as follows, the first-named being the most positive:—silver, platinum, charcoal of *lignum-vitæ*, palladium, gold. In a dilute aqueous solution of the salt, the order found was:—aluminium, magnesium, silicon, iridium, rhodium, and carbon of *lignum-vitæ*, platinum, silver, palladium, tellurium, gold.

The chemical behaviour of the salt was also investigated. In many cases considerable destruction of the platinum vessels occurred, either in the experiments themselves, or in the processes of cleaning the vessels from the products of the reactions.

Hydrogen does not decompose the dry salt, even with the aid of sunlight, nor does a stream of that gas decompose an aqueous solution of the salt; but the dry salt is rapidly and perfectly decomposed by that gas at an incipient red heat, its metal being liberated.

Nitrogen has no chemical effect upon the salt, even at a red heat, nor upon its aqueous solution. Dry ammonia gas is copiously absorbed by the dry salt. In one experiment the salt absorbed about 844 times its volume of the gas. The salt in a fused state is rapidly and perfectly decomposed by dry ammonia gas, and its silver set free. A saturated solution of the salt is also instantly and violently decomposed by strong aqueous ammonia.

Oxygen has no effect either upon the dry salt at 15° C., or at a red heat, nor upon its aqueous solution. Steam perfectly and rapidly decomposes the salt at an incipient red heat, setting free all its silver. No chemical change took place on passing either of the oxides of nitrogen over the salt in a state of fusion.

By passing anhydrous hydrofluoric acid vapour over perfectly anhydrous and previously fused fluoride of silver, at about  $60^{\circ}$  Fahr., distinct evidence of the existence of an acid salt was obtained. This acid salt is decomposed by a slight elevation of temperature.

Numerous experiments were made to ascertain the behaviour of argentic fluoride in a state of fusion with chlorine; and great difficulties were encountered in consequence of the extremely corrosive action of the substances when brought together in a heated state. Vessels of glass, platinum, gold, charcoal, gas-carbon, and purified graphite were employed\*. By heating the salt in chlorine, contained in closed vessels formed partly of glass and partly of platinum, more or less corrosion of the glass took place, the chlorine united with the platinum and fluoride of silver to form a double salt, and a vacuum was produced. By similarly heating it in vessels composed wholly of platinum, the same disappearance of chlorine, the same double salt, and a similar vacuum resulted. Also, by heating it in vessels composed partly of gold, an analogous double salt, the same absorption of chlorine and production of rarefaction were produced. And by employing vessels partly composed of purified graphite, a new compound of fluorine and carbon was obtained.

“Approximate Determinations of the Heating-powers of Arcturus and  $\alpha$  Lyrae.” By E. J. Stone, F.R.S., First Assistant at the Royal Observatory, Greenwich.

About twelve months ago I began to make observations upon the heating-power of the stars. My first arrangements were simply these:—I made use of a delicate reflecting astatic galvanometer, and a thermo-electric pile of nine elements. The pile was screwed into the tube of a negative eyepiece of the Greenwich Great Equatorial, from which the eye-lenses had been removed.

I soon convinced myself that the heat, condensed by the object-glass of twelve and three-quarters inches upon my pile, was appreciable in the case of several of the brighter stars; but the endless changes in the zero-point of the galvanometer-needle, and the magnitude of these changes, compared with those arising from the heating-power of the stars, prevented me from making any attempts to estimate the absolute magnitude of the effects produced. Every change in the state of the sky, every formation or dissipation of cloud, completely drove the needle to the stops.

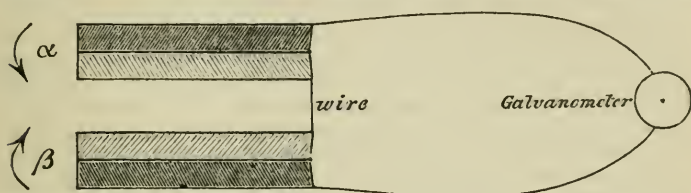
At the February Meeting of the Royal Astronomical Society I first became aware of what Mr. Huggins had done upon this question. His arrangements, however, did not appear to me to meet the difficulties which I had encountered. After some trials, I arranged my apparatus as follows, and with its present form I am satisfied.

$\alpha$  and  $\beta$  are two pairs of plates of antimony and bismuth. The areas are about  $(0.075)^2$  inch, and their distance is about 0.25 inch.

The poles are joined over in opposite directions to the terminals of the pile and galvanometer. The whole pile is screwed into a tube

\* In the next communication will be described the results obtained with vessels formed of other materials.

of one of the negative eyepieces of the great equatorial. This completely shuts the pile up in the telescope-tube. A thick flannel bag



is then wrapped over the eyepiece and terminals. The bag is prevented from actually touching the case of the pile, and is useful in preventing the irregular action of draughts upon the case of the pile and terminals. The wires are led from the terminals of the pile to the observatory library, where I have placed the reflecting galvanometer. This separation of the galvanometer from the telescope is most inconvenient; but it was absolutely necessary, on account of the large moving masses of iron in the observing-room.

The two faces  $\alpha$  and  $\beta$  of the pile are so nearly alike, that the resultant current generated by any equal heating of them is exceedingly feeble.

The telescope is first directed so that the star falls between the faces  $\alpha$  and  $\beta$ , and allowed to remain thus until the needle is nearly steady at the zero.

The star is then placed alternately upon the faces  $\alpha$  and  $\beta$ , and the corresponding readings of the galvanometer taken as soon as the needle appears to have taken up its position, which usually takes place in about ten minutes. In order to avoid changes of zero, I have always reduced those readings by comparing a reading with star on face  $\alpha$  with the mean of two readings with star on  $\beta$ , taken before and after the reading with star on  $\alpha$ , or *vice versa*.

With this precaution I have never met with any anomalous results, although in making the observations I have usually joined over the terminals without knowing the direction for heat, and have left this undetermined until the completion of the observations. I mention this because the differences in the readings for star on  $\alpha$  and star on  $\beta$  in the state in which I use my galvanometer are small.

On many nights, when very slight appearances of cloud prevailed, I have not been able to make any satisfactory observations at all.

The number of divisions over which the spot of light travels on the galvanometer-scale for a given difference of temperature of the faces  $\alpha$  and  $\beta$  is of course dependent upon many circumstances, and especially upon the position of the sensitiveness-regulation magnet of the galvanometer.

I have thought it useless, therefore, to publish any results unless obtained upon nights when the state of the galvanometer was eliminated by referring to an independent source of heat. The way in which this has been attempted is as follows:—

After obtaining the differences in the position of the spot of light



on the galvanometer-scale for star on  $\alpha$  and star on  $\beta$ , I remove the pile from the telescope, leaving all its galvanic connexions untouched, and mount the pile so that, of the two halves of the face of a Leslie's cube containing boiling water, each radiates heat upon one face,  $\alpha$  or  $\beta$  of the pile, placed at a known distance of about twenty inches from the cube. After some time the deflection of the needle will fall nearly to zero, and become steady enough for observation. A piece of glass,  $G$ , is then placed to intercept from  $\beta$  a portion of the heat radiating from one half of the face of the cube, and when the needle has taken up its position, the reading is taken. Next the glass  $G$  is placed to intercept a portion of the heat from the face  $\alpha$ , and the galvanometer-reading taken, as before, as soon as the needle has assumed its position of rest.

If, then,  $\theta$  is the mean difference of readings for star on face  $\alpha$  and face  $\beta$ ,  $\phi$  the mean difference for glass before  $\beta$  and  $\alpha$ ,  $C$  the heating-power of each half of the cube at its distance from the faces of the pile, and  $p$  the measure of the absorption of the piece of glass  $G$ , then the heating-power of star

$$= \frac{\theta}{\phi} \times C \times p.$$

The quantity  $p$  has been determined by merely comparing the readings of the galvanometer, obtained by cutting off the whole heat from one-half of the cube, with that obtained by intercepting a portion of this heat by the glass  $G$ . A considerable number of accordant results gave  $p=0.725$ .

To determine the quantity  $C$ , I have proceeded as follows :—

1st. I have placed two very delicate thermometers, one in contact with each face  $\alpha$  and  $\beta$  of the pile, along the lines of junction of the plates. The thermometers were separated from each other, and the direct radiation of one on the other prevented by the interposition of a piece of blackened card. The two thermometers, with the faces of the pile in contact, were then exposed to the radiation of the halves of the face of the cube containing the boiling water. A third delicate thermometer was read for registration of any change in the temperature of the surrounding air. This thermometer was protected from the direct radiation from the cube. The pile, with thermometers in contact, was then placed at different distances from the cube and the thermometer-readings taken. I have usually taken readings at three distances—one at about 23.5 inches, another at 11.9 inches, another at 2.5 inches. From a comparison of these readings with those taken before the heat from the cube fell upon the thermometers, I infer the heating-power of each half of the cube upon the thermometers, with the faces of the pile in contact. Calling this quantity for one inch of distance  $H'$ , I find for my cube in its present state, with slightly lacquered face,  $H'=130^\circ \text{ F}$ .

2nd. If  $H$  denote the corresponding heating-power of each half of the cube upon the faces of the pile  $\alpha$  and  $\beta$ , I have found the ratio  $H : H'$  as follows :—

The thermometers being placed in contact with the faces of the pile, and the galvanic connexions made, we may be certain that the

temperature of the thermometers has been imparted to the faces of the pile when the needle is steady, provided that the current be carried from the thermometers without loss in the nature of increased resistance. I have therefore compared the deviations produced by glass G before the faces  $\beta$  and  $\alpha$  with the thermometers in contact and without thermometers in contact with two different amounts of resistance in circuit. Such observations have been considered satisfactory only when the two resistances for thermometers in contact and without thermometers were sensibly equal. This condition can be obtained by making the thermometers touch along the lines of junction of the antimony and bismuth; but the connexion being one of mere contact, there is always danger of failure.

The following observations were made on 1869, Aug. 19 :—

1. Without thermometers :

Resistance =  $R + 0.003$  B.A. unit.

Mean difference, G before  $\beta$  — G before  $\alpha$  = 735 div.

2. With thermometers in contact :

Resistance =  $R_1 + 0.003$  B.A. unit.

Mean difference = 698 div.

3. With thermometers in contact :

Resistance =  $R_1 + 1.437$  B.A. unit.

Mean difference = 324 div.

4. Without thermometers :

Resistance =  $R + 1.437$  B.A. unit.

From (1) and (4)  $R = 1.251$  B.A. unit.

From (2) and (3)  $R_1 = 1.239$  B.A. unit.

The resistances are therefore each sensibly equal to 1.245 B.A. unit.

From (1) (2) and (3) (4) we find  $\frac{H}{H_1} = 1.056$ .

From the mean of such determinations I find

$$\frac{H}{H_1} = 1.087.$$

If, therefore,  $c$  is the distance of the pile from the cube in inches, we have

$$C = \frac{130^\circ}{c^2} \times 1.087.$$

And the heating-power of the star

$$= \frac{130^\circ}{c^2} \times 1.087 \times 0.725 \times \frac{\theta}{\phi}.$$

I may mention that the whole area of a face of the small pile may be considered effective in the focus of the equatorial.

The following observations have been made and reduced as above.

1869. Aug. 2.

Observations of *Arcturus*, altitude about  $25^\circ$ .

$\theta = 23$  div.

$\phi = 160$  div.

$c = 17.6$  inches.

Heating-power of star

$$= \frac{130}{(17.6)^2} \times 1.087 \times 0.725 \times \frac{23}{160} \left( \frac{17}{37.5} \right) \\ = 0^{\circ}.0216 \text{ F.}$$

For the observations  $\phi$  the scale was removed nearer the galvanometer, so that the effective radius for these readings was  $2 \times 17$  inches against  $2 \times 37.5$  inches for the observations of the star.

1869. August 11.

Observations of Arcturus.

$$\theta = 27 \text{ div.}$$

$$\phi = 114 \text{ div.}$$

$$c = 24 \text{ inches.}$$

Effective radius for observations, 32 inches.

Heating-power of Arcturus

$$= \frac{130}{(24)^2} \times 1.087 \times 0.725 \times \frac{27}{114} \times \frac{32}{75} \\ = 0^{\circ}.0180 \text{ F.}$$

The mean result of the observations on these two nights is

$$0^{\circ}.0198 \text{ F.}$$

as a measure of the heating-effect of Arcturus in raising the temperature of the plate of antimony and bismuth when the heat is condensed by the object-glass of 12.75 inches.

If the absorption by the object-glass be considered insensible, the direct effect upon the pile will be

$$0^{\circ}.000000685 \text{ F.}$$

I have not yet determined the coefficient of absorption for the object-glass; but if it be provisionally taken at  $\frac{1}{2}$ , the direct heating-effect of Arcturus

$$= 0^{\circ}.00000137 \text{ F.}$$

The result may be otherwise stated as follows:—That the heat received from Arcturus is sensibly the same as that from the whole face of the cube containing boiling water at 400 yards.

1869. August 14.

Observations of  $\beta$  Lyræ at 8<sup>h</sup> 38<sup>m</sup> G.M.T.

$$\theta = 15 \text{ div.}$$

$$\phi = 686 \text{ div.}$$

Heating-power for  $\beta$  Lyræ

$$= \frac{130}{(24)^2} \times 1.087 \times 0.725 \times \frac{15}{686} = 0^{\circ}.0039 \text{ F.}$$

Observations were subsequently made of  $\alpha$  Lyræ; but the zero was unsteady, and as the night advanced clouds appeared and ultimately interrupted the observations.

1869. August 14.

$\alpha$  Lyræ. Star on  $\alpha$  — star on  $\beta$  = 11 div.

1869. August 15.

The night was very clear, and the air steady, but completely saturated with moisture, at a temperature of about  $52^{\circ}$ . The mean of fourteen observations of the difference of reading for  $\alpha$  Lyræ on  $\alpha$  and  $\beta$  gave only 11 divisions. I have no doubt that the small effect here obtained was due principally to the amount of moisture in the air.

1869. August 25.

Observations of  $\alpha$  Lyræ. Night fine.

Mean value of the difference from nine observations was

$$\theta = 33 \text{ div.}$$

$$\phi = 669 \text{ div.}$$

$$c = 24 \text{ inches ;}$$

$$\therefore \text{ heating-power of } \alpha \text{ Lyræ} = 0^{\circ} \cdot 0088 \text{ F.}$$

This result is again so much smaller than those obtained from Arcturus, although the observations of Arcturus were made under more unfavourable circumstances with respect to altitude, that I cannot but regard it as a fact that the star Arcturus does give us more heat than  $\alpha$  Lyræ,—a result probably due to the same cause which gives rise to the difference in colour between these stars, viz. the greater absorption of the red end of the spectrum in the case of  $\alpha$  Lyræ than in the case of Arcturus.

I may here mention that on June 25, 1869, I made a direct comparison between Arcturus and  $\alpha$  Lyræ. The result gave for the heat received from Arcturus : that from  $\alpha$  Lyræ :: 3 : 2 ; but on account of the observations of  $\alpha$  Lyræ having been interrupted by cloud, they were not sufficiently numerous to eliminate mere errors of reading.

From the whole of these observations I think we may conclude :—that Arcturus gives to us considerably more heat than  $\alpha$  Lyræ ; that the amount of heat received is diminished very rapidly as the amount of moisture in the air increases ; that nearly the whole heat is intercepted by the slightest cloud ; that, as first approximations, the heat from Arcturus at an altitude of  $25^{\circ}$ , at Greenwich, is about equal to that from a three-inch cube containing boiling water at a distance of 400 yards.

The heat from  $\alpha$  Lyræ at an altitude of  $60^{\circ}$  is about equal to that from the same cube at a distance of about 600 yards. The form given to the pile appears likely to be useful in many inquiries respecting differences of heating-power.

Jan. 20.—Dr. William A. Miller, Treas. and V.P., in the Chair.

The following communication was read :—

“ Preliminary Paper on certain Drifting Motions of the Stars.”

By Richard A. Proctor, B.A., F.R.A.S.

A careful examination of the proper motions of all the fixed stars in the catalogues published by Messrs. Main and Stone (Memoirs of the Royal Astronomical Society, vols. xxviii. and xxxiii.) has led me to a somewhat interesting result. I find that in parts of the heavens the stars exhibit a well-marked tendency to drift in a definite direction. In the catalogues of proper motions, owing to the way in which the



stars are arranged, this tendency is masked ; but when the proper motions are indicated in maps, by affixing to each star a small arrow whose length and direction indicate the magnitude and direction of the star's proper motion, the star-drift (as the phenomenon may be termed) becomes very evident.

It is worthy of notice that Mädler, having been led by certain considerations to examine the neighbourhood of the Pleiades for traces of a community of proper motion, founded on the drift he actually found in Taurus his well-known theory that Aleyone (the *lucida* of the Pleiades) is the common centre around which the sidereal system is moving. But in reality the community of motion in Taurus is only a single instance, and not the most striking that might be pointed out, of a characteristic which may be recognized in many regions of the heavens. In Gemini and Cancer there is a much more striking drift towards the south-east, the drift in Taurus being towards the south-west. In the constellation Leo there is also a well-marked drift, in this case towards Cancer.

These particular instances of star-drift are not the less remarkable that they (the stars) are drifting almost exactly in the direction due to the proper motion which has been assigned to the sun, because the recent researches of the Astronomer Royal have abundantly proved that the apparent proper motions of the stars are not to be recognized as principally due to the sun's motion. Mr. Stone has shown even that we must assign to the stars a larger proper motion, on the average, than that which the sun possesses. Looking, therefore, on the stars as severally in motion, with velocities exceeding the sun's on the average, it cannot but be looked upon as highly significant that in any large region of the heavens there should be a community of motion such as I have described. We seem compelled to look upon the stars which exhibit such community of motion as forming a distinct system, the members of which are associated indeed with the galactic system, but are much more intimately related to each other.

In other parts of the heavens, however, there are instances of a star-drift opposed to the direction due to the solar motion. A remarkable instance may be recognized among the seven bright stars of Ursa Major. Of these, the stars  $\beta$ ,  $\gamma$ ,  $\delta$ ,  $\epsilon$ , and  $\zeta$  are all drifting in the same direction, and almost exactly at the same rate, towards the "apex of the solar motion," that is, the point from which all the motions due to the sun's translation in space should be directed. If these five stars, indeed, form a system (and I can see no other reasonable explanation of so singular a community of motion), the mind is lost in contemplating the immensity of the periods which the revolutions of the components of the system must occupy. Mädler had already assigned to the revolution of Alcor around Mizar ( $\zeta$  Ursæ) a period of more than 7000 years. But if these stars, which appear so clear to the naked eye, have a period of such length, what must be the cyclic periods of stars which cover a range of several degrees upon the heavens ?

In like manner the stars  $\alpha$ ,  $\beta$ , and  $\gamma$  of Arietis appear to form a single system, though the motion of  $\alpha$  is not absolutely coincident

either in magnitude or direction with that of  $\beta$  and  $\gamma$ , which are moving on absolutely parallel lines with equal velocity.

There are many other interesting cases of the same kind. I hope soon to be able to lay before the Society a pair of maps in which all the well-recognized proper motions in both hemispheres are exhibited on the stereographic projection. In the same maps also the effects due to the solar motion are exhibited by means of great circles through the apex of the solar motion, and small circles or parallels having that apex for a pole.

It appears to me that the star-drift I have described serves to explain several phenomena which had hitherto been thought very perplexing. In the first place, it accounts for the small effect which the correction due to the solar motion has been found to have in diminishing the sums of the squares of the stellar proper motions. Again, it explains the fact that many double stars which have a common proper motion appear to have no motion of revolution around each other; for clearly two members of a drifting-system might appear to form a close double, and yet be in reality far apart and travelling not around each other, but more closely around the centre of gravity of the much larger system they form part of.

I may add that, while mapping the proper motions of the stars, I have been led to notice that the rich cluster around  $\chi$  Persei falls almost exactly on the intersection of the Milky Way with the great circle which may be termed the equator of the solar motion—that is, the great circle having the apex of the sun's motion as a pole. This circumstance points to that remarkable cluster, rather than to the Pleiades, as the centre of the sidereal system, if, indeed, that system have a centre cognizable by us. When we remember that for every fixed star in the Pleiades there are hundreds in the great cluster in Perseus, the latter will seem the worthier region to be the centre of motion. I should be disposed, however, to regard the cluster in Perseus as the centre of a portion of the sidereal system, rather than as the common centre of the Galaxy.

The peculiarities of the apparent proper motions of the stars seem to me to lend a new interest to the researches which Mr. Huggins is preparing to make into the stellar proper motions of recess or approach.

#### GEOLOGICAL SOCIETY.

[Continued from p. 315.]

May 26th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President,  
in the Chair.

The following communications were read:—

1. "Notes on the Geology of Cape-York Peninsula, Australia."

By Alex. Rattray, M.D.

The author stated that the Eastern mountain-range of Australia is produced through and forms the axis of the peninsula of Cape York; it consists of various granites and porphyries, gneiss, and

felspathic and quartzose rocks. In Cape York itself the rock is a porphyry, with numerous crystals of yellowish quartz. Resting on the flanks of this axis are beds of sandstone, regarded as of Carboniferous age by the Rev. W. B. Clarke, and referred to the Oolite by Prof. M'Coy. The surface-rock in the neighbourhood of Cape York is an ironstone, varying from a light friable clay to a dense ferruginous conglomerate, sometimes nodular and magnetic; an average specimen contained 39.69 per cent. of iron. This deposit extends at least as far south as the Mitchell River, on the west side of the peninsula, and as far as Weymouth Bay on its east side. It is regarded as Posttertiary by Mr. Clarke. Between this and the igneous rock intervenes a local deposit of coarse quartzose sandstone, which forms bold cliffs in Albany Island and on the opposite coast of the mainland. No boulder-deposits or glacial markings were detected by the author.

The author referred to the occasional occurrence of earthquakes in Eastern Australia, and to the evidence that at least the north-east of that continent is being slowly elevated—namely, the occurrence of water-worn caves above high-water mark in Albany Island and on the opposite coast, the existence of extensive plains covered with sand-dunes, especially towards the north of the peninsula, and the gradual emergence of islands in the line of the great Barrier Reef.

2. "On the Formation of the Chesil Bank, Dorset." By H. W. Bristow, Esq., F.R.S., F.G.S., and Wm. Whitaker, Esq., B.A., F.G.S.

The authors first described the general character of the bank, and noticed the previous hypotheses that have been suggested to account for its formation. They maintained that it had been formed as an ordinary shingle-beach banked up against the land, and afterwards separated by the wearing action of the small rivers flowing to the sea at this part. They stated that west of the Chesil Bank there are cliffs of greater or less elevation, through which small streams flow down to the shore, when they turn a little eastward between the beach and the land, and then filter through the shingle without breaching it. The effect of a similar river-action on the low shore behind the Chesil Bank would, in the authors' opinion, be to cut such a channel as that of the Fleet, by which this bank is separated from the shore.

3. "On a Raised Beach at Portland Bill, Dorset." By W. Whitaker, Esq., B.A., F.G.S.

The author stated that a deposit of shingle occurs upon the cliffs of the south-east part of the Isle of Portland, extending from Cave Hole to the Bill. At the former place it is from 30 to 40 feet above the sea, and is capped by a considerable thickness of angular "head." Further south there is less of the "head." The shingle consists of pebbles of limestone, flint, and chert; and the deposit contains shells of *Littorina littorea* and *littoralis*, *Patella vulgata*, and other species. Near the beacon these species, with *Purpura lupillus*, occur. To the west of the Bill the shingle is partly covered by a

yellowish-brown loam, containing land and freshwater shells, such as *Bithynice* and *Pupæ*.

4. "On the occurrence of *Terebratula diphya* in the Alps of the Canton de Vaud." By E. Tawney, Esq., F.G.S., with a Note by T. Davidson, Esq., F.R.S., F.G.S.

The author recorded the occurrence of *Terebratula diphya* in a block of Oxfordian limestone derived from the ridge of the Vanil Noir, near Paray Charbon. No Neocomian beds occur within a distance of several miles. The block had merely fallen, and had not been transported. The author indicated that this result is in opposition to the views of M. Hébert, who maintains the identity of *T. diphya* and *T. diphyoides*, and refers all the beds containing these fossils to the Neocomian series.

Mr. Davidson, in his note, indicated the distinctive characters of *T. viator*, Piet. (= *diphya*) and *T. diphyoides*, and confirmed the author's opinion as to its Jurassic age.

5. "On a new *Labyrinthodont* from Bradford." By T. H. Huxley, LL.D., F.R.S., President, with a Note on its locality and stratigraphical position, by Louis C. Miall, Esq.

The *Labyrinthodont* nature of this fossil was said to be proved by the characters of the vertebræ, ribs, and ventral armour. It exhibits portions of both jaws, which bear close-set teeth of nearly equal size, nearly circular in section, and slightly recurved at the apex, which is obtusely pointed. The surface of the upper jaw shows a pitted sculpture. The ventral armour consists of oval plates, traversed obliquely by a convex ridge, dividing them into two unequal parts. They are supposed by the author to have overlapped each other so as to show little more than the surfaces of the oblique ridges. The author stated that this Amphibian appears to be most nearly related to his genus *Pholidogaster*, but that it differs therefrom in the form of the vertebral centra, and in the nature of the ventral armour. He proposed to give it the new generic name of *Pholiderpeton*.

Mr. Miall stated that the fossil described by Prof. Huxley was obtained from the Black Bed or Royd's Coal at Toftshaw, near Bradford, and remarked that the Amphibians of the Coal-measures appear to have been not only specifically numerous, but individually abundant. He also noticed the difficulty of defining exact horizons within the Coal-measures.

6. "On the Maxilla of *Megalosaurus*." By T. H. Huxley, LL.D., F.R.S., President.

The author stated that the specimen which he was about to describe, belonging to Mr. Abbay, of King's College, was the first example of the upper jaw of *Megalosaurus* that had ever been seen. It showed the left side of the jaw, measured nearly 18 inches in length, and  $4\frac{1}{4}$  inches in depth anteriorly; the posterior part was produced into a thin and probably free process. The author described the general form of the nasal and orbital openings, as indicated by the characters of the specimen, and stated that the anterior part of the jaw exhibits no suture indicating the separation of the præmaxilla from the maxilla. The teeth were stated to be few in number, but



very large, the part concealed by the jaw being exceedingly long. One tooth, exposed in its whole length by the breaking away of the bone, measured 6·4 inches, of which the crown formed 2·6 inches.

June 9th, 1869.—Sir R. I. Murchison, Bart., F.R.S., V.P.G.S., in the Chair.

The following communications were read:—

1. “Notes on the Sutherland Gold-fields.” By the Rev. J. M. Joass.

2. “Observations on the ‘Nuggetty Reef,’ Mount Tarrangower Gold-field.” By Dr. G. H. F. Ulrich, F.G.S.

The author commenced by indicating the position and characters of the so-called “Nuggetty Reef,” which is situated about  $2\frac{1}{2}$  miles N.N.W. of the little town of Maldon, about 87 miles N.W. of Melbourne, and consists of two strong quartz veins, separated by a mass of bluish-grey metamorphic sandstone. In certain places the “reef” is cut transversely or obliquely by granite bars or veins, of which the author described four, and stated that, although some of them are, and all probably have been, connected with the main mass of granite, they present rather the appearance of zones of impregnation than of intrusive dykes—there being no line of separation between the quartz rock and the granite, but felspar and black mica make their appearance in the quartz, and gradually increase in quantity until the granite band is formed. The author noticed the following minerals as associated with the gold in the “Nuggetty Reef:”—Iron pyrites, arsenical pyrites, magnetic pyrites, copper pyrites, galena, zinc-blende, and a compound of gold and bismuth, to which he gave the name of *Maldonite*.

3. “On the Caratal Gold-field.” By Dr. C. Le Neve Foster, F.G.S.

The author stated that the Caratal Gold-field is situated about 100 miles south of the Orinoco, at a point about 75 miles above its principal mouth. The rock of the district is chiefly gneiss, with some mica-schist, hornblende schist, and granite. The gold occurs in four ways:—1, in lodes, veins, &c.; 2, in alluvial or “placer” diggings; 3, in red earth or “Tierra de flor;” and, 4, in gravel and sand of river-beds. The mode of occurrence of the gold in these different places, and the various processes employed in collecting it, were described by the author, who estimates the quantities of the precious metal obtained in the Caratal district as follows:—

In 1866.....	15,587 oz.
1867.....	30,142 oz.
1868 (9 months) ..	22,481 oz.

The greater part is obtained from the lodes.

4. “On the Geology of Guyana in Venezuela.” By Ralph Tate, Esq., Assoc. Linn. Soc., F.G.S.

The author prefaced his remarks by pointing out the leading physical features of Guyana in Venezuela, and by alluding to the Geology of Venezuela to the north of the river Orinoco, as determined by Mr. Wall.

Mr. Tate has ascertained the existence of an extensively developed metamorphic series, consisting of felstones, tale schists, quartzites, gneiss of great variety, hornblende slate, and amphibole

rock, striking about N.E. and S.W., and dipping generally to the N.W. at a high angle. The series exhibits little disturbance, but great lithological variation; on the whole felspar predominates in the lower part, whilst hornblende increases in quantity as we ascend in the section. The felstone and talc schist, more especially the former, contain eminently auriferous lodes; the gneiss contains disseminated gold, and that to the north of Upata a rich deposit of red hæmatite and a few intrusions of greenstone.

This metamorphic series was considered to be contemporaneous with a similar series constituting the Littoral Cordilleras of North Venezuela, the præeretaceous age of which is proved by their being overlain by unaltered Neocomian beds. Indulging in speculation as to the age of these auriferous rocks, the author sought, by analogy of their mineral contents and their lithological similarity to certain auriferous and associated strata in Bolivia, to establish their Silurian age.

Skirting the Orinoco and abutting against the escarpment of the Itacama Mountains, were described the "Llanos" sandstones and conglomerates, derived from the metamorphic series. These beds, which were referred to the Upper Miocene, are in their easterly extension lignitiferous and asphaltic; and as the basin of the Orinoco is excavated in them, it was contended that the present river could have played no part in the accumulation of the materials from which the asphaltic deposits have been supposed to have originated.

5. "On the Nature and Cause of the Glacial Climate." By Jos. John Murphy, Esq., F.G.S.

The author cited the conclusions arrived at by Mr. Croll as to the causes of glacial climate, and stated his agreement with them, except in one instance: he maintained, in opposition to Mr. Croll, that the glaciated hemisphere must be that in which the *summer* occurs in *aphelio* during the period of greatest eccentricity of the earth's orbit. He showed that a cool summer had more to do with the prevalence of glacial conditions than a cold winter, and referred to several phenomena furnishing arguments in favour of his opinion.

June 23rd, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On two new Species of *Gyrodus*." By Sir Philip de Malpas Grey Egerton, Bart., M.P., F.R.S., V.P.G.S.

The author remarked upon the characters of the genus *Gyrodus*, of which he described two new species, namely:—*G. Goweri*, from a deposit of Oolitic age on the east coast of Sutherland, having the scales covered with a somewhat reticulated raised pattern, interspersed with granules; and *G. coccoderma*, from the Kimmeridge Clay of Kimmeridge, having the scales adorned with a multitude of symmetrical granules which show no tendency to coalesce. The author also described a vomer of *Sphærodus gigas*, bearing teeth of the form usual in that genus, and remarked that this specimen established the validity of the genus *Sphærodus*.

2. "Note on a very large Saurian Humerus from the Kimmeridge Clay of the Dorset coast." By J. W. Hulke, F.R.S., F.G.S.

This stupendous limb-bone, 31 inches long, was obtained from Kimmeridge Bay by J. C. Mansel, Esq. It had a subcylindrical shaft, a transversely elongated proximal, and a cubical distal extremity. The distal end is mapped out by a wide shallow posterior groove, and a narrower but deeper anterior notch, into a couple of condyles, of which the inner or posterior is the larger. The anterior border of the shaft towards the proximal end rises, as if to form a deltoid crest. The cortical tissue of the shaft is remarkably dense and polished. There is not any medullary cavity; but the interior of the cortex is filled with cancellous tissue.

The author pointed out that the form of the terminal surfaces removed the bone from all the Enaliosaurians, and brought it into close relation to the humerus of existing Crocodilians, from which, however, it differed in its being less curved and by its great size.

He next referred to its differences from the humerus of the Dinosaurs *Iguanodon* and *Hylæosaurus*, and remarked that it most nearly resembled the large limb-bone on which Mantell founded his genus *Pelorosaurus*.

3. "Note on some fossil remains of a Gavial-like Saurian from Kimmeridge Bay, establishing its identity with Cuvier's 'Deuxième Gavial d'Honfleur' and with Quenstedt's *Dakosaurus*." By J. W. Hulke, Esq., F.R.S., F.G.S.

The fossils which formed the subject of this communication were also collected by Mr. Mansel in Kimmeridge Bay. They demonstrated the existence of a Saurian having long, subincurved, subretrocurved, laterally compressed, unequally convex teeth, with an anterior and a posterior finely serrated edge, loosely implanted in distinct and separate sockets, and vertically replaced by young teeth rising into the base of the large open pulp-cavity of the fang of the old tooth.

The lower jaw, of which the greatest part of the right half is preserved, is about 40 inches long. The symphysis is very long, and includes the opercular bone. The upper jaw shows a terminal, undivided nostril, not inflated laterally as in the *Teleosaurus*. The lines of the jaws seem to merge into the cranium less abruptly than in the living Gavial, which gives the outline of the head a greater resemblance to *Mecistops*. The vertebræ are biconcave, and the outer surface is hollowed and somewhat overhung by the roundish articular surfaces. The transverse processes are long and directed outwards, and slightly backwards and downwards; their posterior border thick, the anterior thin. The ribs have bifurcated spinal ends. The femur, 14 inches long, resembles that of the living Crocodilians, only it is less twisted.

The structure of the jaw, the form and attachment of the teeth, and the manner of their succession, with the form of the vertebræ and ribs, proved this Kimmeridge Saurian to be a Crocodilian (and not a Lacertilian) resembling a bastard Gavial. The author next demonstrated its identity with *Dakosaurus*, Quenstedt, and hence with *Geosaurus maximus*, Plieninger; and he expressed a belief that it was also generically identical with Cuvier's Honfleur Gavial

“tête à museau plus court,” and Geoffroy St. Hilaire’s *Steneosaurus rostro-minor*.

4. “On the Geology of a Portion of Abyssinia.” By William T. Blanford, Esq., F.R.S. &c.

This paper contained a brief description of the principal geological observations made by the writer when accompanying the late Abyssinian expedition. After referring to the notices by previous explorers, he gave a list of the different formations met with, viz. :—  
1. Recent Soils on the highlands, including regur or cotton-soil similar to that found in India, and alluvial deposits on the coast.  
2. The volcanic series which skirts both coasts of the southern portion of the Red Sea. This, which was poorly developed on the west coast of Annesley Bay, it was proposed to designate the Aden volcanic series.  
3. The great trappean formations of the Abyssinian highlands, consisting of two groups unconformable to each other—(1) the Magdala group, consisting of trachytes and dolerites, and (2) the Ashangi group, entirely composed of dolerites—both of great thickness and formed of bedded volcanic rocks, lavas, and ashes.  
4. The Antalo limestone of Oolitic age, containing *Ceromya similis*, *Trigonia costata*, and other characteristic forms.  
5. Adegrat sandstone, a massive formation occupying a considerable area in Northern Tigre, and perhaps representing the coal-bearing rocks known to exist north-west of Lake Dembea, but unfossiliferous.  
6. Metamorphics of varying mineral character, having a general north and south strike, due to preexistent cleavage. Some brief remarks on denudation &c. followed.

5. “On the Graphite of the Laurentian of Canada.” By Prof. J. W. Dawson, LL.D., F.R.S., F.G.S.

The author described the modes of occurrence of great quantities of graphite in the Laurentian limestones of Canada. He regarded the presence and characters of this mineral as indicative of the existence of plants, side by side with *Eozoon*, at the period of the deposition of these limestones.

6. “On the Correlation, Nature, and Origin of the Drifts of North-west Lancashire and part of Cumberland.” By D. Mackintosh, Esq., F.G.S.

In this paper the author stated the results of observations made last December, January, and February, during prolonged visits to Blackpool, Ulverstone, and Lancaster. At Blackpool repeated examinations of the drifts, as exposed on the tidal zone and in the cliffs, convinced him that there is a distinct triplex series—a hard, very stony lower boulder-clay, a middle sand and gravel, and a less stony upper boulder-clay and marl. At Blackpool the author likewise found a newer deposit of warp-clay (or Scotch *slutsh*) interstratified with and overlain by peat and sand. On the Furness side of Morecambe Bay, the same lower boulder-clay, there called “pinel,” made its appearance. After many visits to the section near the Ulverstone railway-station, he convinced himself that the pinel not only contains seams of sand, but overlies a thick bed of finely laminated sand. It wedges out upwards into an exceedingly con-



torted and false-bedded mass of middle sand and gravel. The pinel he found running up the hill-sides to an altitude of 800 feet. Above Soutergate, at a height of between 700 and 800 feet, he found a distinct line of demarcation between the pinel and an upper boulder-loam which lower down contained boulders of granite. The paper contained a particular description of the completely isolated island-hill called Dunnerholme with its capping of upper boulder-clay and rounded pebbles, many of them still sticking in the funnel-shaped cavities of the underlying limestone, which they apparently helped to grind out. The author described a great thickness of upper boulder-clay near Barrow, and defined the north-east boundary of the granitic drift, which he believed could not all have come from Eskdale in Cumberland. He likewise described the naturally sculptured large boulder-stones of Stainton. On the west side of the Duddon, he could clearly trace the pinel and middle sand and gravel, and, he believed, an upper rubbly boulder-clay, the lower limit of which, however, was not well defined. He found all the drifts, but especially the upper, spreading out along the base of Blackcombe in the shape of a regular terrace running up into its combs, and choking up its gorges. He described the appearances presented and positions occupied by the drifts at high levels on Blackcombe, and endeavoured to trace a distinction between marine drifts, glacial moraine matter, and ordinary "scree" or fallen débris. The author gave a particular account of the enormous mounds of sand and gravel between Lancaster and Carnforth, described the mode of occurrence and striation of the limestone boulders, and traced the latter to the action of sea-ice on old sea-coasts around Warton Hill, &c. In conclusion, he considered the mode of accumulation and derivation of the drifts of North-west Lancashire, and compared them with the lower boulder-clay, middle sand and gravel, and upper boulder-clay of the splendid section, 150 feet high, near Preston, and with the drifts of other districts.

7. "On the Connexion of the Geological Structure and Physical Features of the South-east of England with the Consumption Death-rate." By W. Whitaker, Esq., B.A., F.G.S.

The author stated that his investigation of this subject, which was carried on in conjunction with Dr. Buchanan, was suggested by the fact that improved drainage had been found to exert a marked influence upon the average number of deaths by consumption in certain districts. The chief result arrived at by an examination of fifty-eight registration districts in Kent and Sussex was, that "wetness of the soil is a great cause of consumption;" and this depends not only upon the perviousness or imperviousness of the soils, but upon their position as regards elevation and slope.

8. "On the Volcanic Phenomena of Hawaii." By the Rev. C. G. Williamson.

In this paper the author gave a detailed account of the general physical features of the island of Hawaii, noticing especially the characters presented by the volcanic mountains of the island and the results of their eruptions. The eruptions and earthquakes of

1868 were particularly described by the author from his personal observations.

9. "Notes on certain of the Intrusive Igneous Rocks of the Lake-district." By Dr. H. A. Nicholson, F.G.S.

The rocks referred to in this paper were the syenite of the Vale of St. John, the syenitic porphyry between Ennerdale and Buttermere, and the felstone porphyry of Carrock Fell. The author described the characters and position of these rocks with regard to the Skiddaw Slates on the one hand, and the green slates and porphyries on the other, and indicated that the latter series of rocks are unconformably deposited upon the purely sedimentary Skiddaw Slates. The Skiddaw Slates were said to be metamorphosed where they come into contact with the intrusive masses, and the latter were regarded by the author as most probably "the roots of the ancient vents from which were derived the alternating ashes and traps which together compose almost the whole of the green-slate series."

10. "On the Fossil Myriopods of the Coal-formation of Nova Scotia and England." By Samuel H. Scudder, Esq.

In this paper the author discussed and described the species of Chilognathous Myriopods which have been detected in the Coal-measures. Of these he recognized 6, viz. *Xylobius sigillarie* (Daws.), *X. similis*, sp. n., *X. fractus*, sp. n., *X. Dawsonii*, sp. n., *X. Woodwardii* (= *sigillarie*, Woodw.), and a species upon which he founded a new genus, *Archiulus xylobioides*. He regarded these forms as constituting a peculiar family, for which he proposed the name of *Archiulidae*.

11. "On the Geology of the Country surrounding the Gulf of Cambay." By Alexander Rogers, Esq., F.G.S., Bombay Civil Service.

The author described the surface of the country as consisting chiefly of deep alluvial soils, derived from the denudation of the primary and metamorphic rocks surrounding the district, the former making their appearance in groups of isolated peaks, projecting, as it were, from a sea of alluvium. The author considered that this alluvium could not have been produced by the action of the existing rivers, and suggested that the Indus may formerly have flowed into the sea by the Gulf of Cambay, the land at the same time being much depressed below its present level. He indicated the evidence in favour of this view furnished by various facts in the geology of the district, and referred especially to the mode of occurrence of laterite.

12. "On a new Acrodont Saurian from the Lower Chalk." By James Wood Mason, Esq., F.G.S., of Queen's College, Oxford.

The author described this reptile, for which he proposed the name of *Acrodontosaurus Gardneri*, as differing from *Mosasaurus* in the apparently persistent distinctness of the præmaxillæ and their small development in the middle line, in the more anterior position of the nasal aperture, which is directed upwards and forwards, in the total obliteration of the maxillo-præmaxillary suture, and in the absence from the cylindrical teeth of opposite denticulated ridges. The

specimen was obtained from the Lower Chalk of Folkestone, about 10 feet above the Chalk Marl.

13. "Rodentia of the Somerset Caves." By W. Ayshford Sanford, Esq., F.G.S.

The author has examined the Rodents from the caves of Somersetshire contained in the Taunton Museum, and found that many of them cannot be referred to species hitherto regarded as belonging to the fauna contemporary with the Mammoth in Britain. He enumerated species of *Arvicola* (including *A. glareola*, Schreb., and *A. ratticeps*, Blas.=*Lemmus medius*, Nilson, and a species which may be new, and for which he proposed the provisional name of *A. Gulielmi*), *Lemmus* (*L. norvegicus*, Desm.), *Lagomys* (*L. spelæus*, Owen), *Lepus* (*L. diluvianus*, Piet., *L. timidus*, Linn., *L. hibernicus*, Bell, and *L. cuniculus*, Linn.), *Spermophilus* (*S. erythrogonoides*, Falc.: the citation of *S. citillus* by the author and Mr. Boyd Dawkins is founded on a mistake), and *Cricetus* (*C. songarus*, Pall.).

## LVII. Intelligence and Miscellaneous Articles.

### ON A NEW KIND OF ELECTRICAL DUST-FIGURES.

BY PROFESSOR VON BEZOLD.

IT is a well-known fact that when two glass plates, coated on one side only, are placed with the uncoated sides at a moderate distance apart, while the coated sides are connected with the poles of a Ruhmkorff's coil, a luminous phenomenon is observed as soon as the apparatus is set to work.

This is a case of discharge produced by influence, and taking place between two non-conducting surfaces, just as in Gassiot's tubes; in which the same phenomena are observed as in Geissler's tubes, although the enclosed gas is not in direct contact with the polar wires.

It appeared interesting to investigate whether the consequences of such a discharge produced by induction between insulating surfaces could be made visible by fine powder, in the same manner as Lichtenberg demonstrated the passage of electricity between a metal and an insulator, and as Kundt has recently shown between two metallic electrodes.

My idea was completely confirmed, and it was observed that figures of great regularity may be obtained in the way in question. I will give the principal experiments here.

On the edge of a glass plate with a circular coating underneath, three small pieces of glass of 1.6 millim. thickness were laid, and on these was placed, with its uncoated side downward, a plate quite similar to the first. The lower plate was strewn with lycopodium powder.

When, now, the lower plate was connected with the positive, and the upper with the negative pole of a large Ruhmkorff's coil which was worked by a single element, and the action continued for about a minute, on removing the upper plate it was found that about half the powder had been projected against it, and that it formed very characteristic figures upon each.

The entire upper plate was covered with small circles which were

free from powder and stood out very distinctly against the thickly covered ground ; in many of them there was in the middle a small heap of dust. The diameter of the circles did not much vary generally ; only towards the edge of the coating did they seem somewhat larger.

The lower plate exhibited no places free from powder, but only small solid circles, and upon them little stars.

About the edge of the coatings (that is, upon the corresponding places of the uncoated sides) there was a circle almost free from powder, which was enclosed in a ring which gradually thinned off towards the outside. When the action of the apparatus was continued a little longer, nearly all the powder travelled towards this external ring, and the parts of the plate over the coatings were almost free from dust.

If the two poles be interchanged, the figures become reversed ; that is, the dust-free places are found upon the lower, and the stars upon the upper plate.

If the distance of the two plates is made somewhat greater, by placing on each other several of the above-mentioned pieces of glass, the diameter of the figures also increases, as is seen from the following Table, in which  $d=1\cdot6$  millim. :—

Distance of the plates.	Diameter of the circles.	
	In the centre of the plate.	At the edge of the plate.
$d$ .....	2·1	2·7
$2d$ .....	3·8	4·6
$3d$ .....	4·8	6·4
$4d$ .....	7·2	7·8

Here, as the distance increases, the dust-ring moves wider and wider outwards.

An exchange of the plates for such as are of thinner glass, or a change in the diameter of the coatings, has no influence on the figures ; but it is necessary to alter the duration of action of the apparatus to obtain similar figures under circumstances thus altered.

It may seem surprising that the figures are so similar to Lichtenberg's ; but it is more surprising that on the plate with whose coating the positive pole is connected, and upon which, therefore, from electrical laws, negative electricity must accumulate, the stars appear, while the regular circles are exhibited upon the other plate.

But this apparent contradiction is removed if, instead of the numerous breaks which Ruhmkorff's coil yields, a single one is effected by hand, and thus a single discharge passes through the system.

This being premised, by correctly regulating the inducing current, only a small number of figures are obtained, but these are very beautiful and like Lichtenberg's. For it is observed that both plates, even the upper one, which was not covered with dust, had become coated with a uniform layer of the powder, which was only interrupted by individual figures.

On the plate whose coating is connected with the positive pole these figures are very regular circles free from powder, whose external diameter is pretty well double that of the thickly powdered central spot.

On the other plate are also circles of the external diameter of the



ring of the figure described above. In their middle are very delicate stars, for the most part with four or five rays.

Hence we have here both the positive and the negative Lichtenberg's figures, and upon those plates, too, upon which, from the laws of induction, they ought to be.

The diameters of these figures are independent of the thickness of the plates, of the size of the coatings, and even of the intensity of the inducing battery, provided this does not exceed certain limits, but vary with the distance of the plates.

The influence of this element is shown in the following Table, which contains the mean value of several observations:—

Distance of the plates.	External diameter of the ring.	Internal diameter of the ring.
$d$ .....	2·1	4·0
$2d$ .....	3·8	7·7
$3d$ .....	5·5	11·0
$4d$ .....	9·3	14·5

An increase in the strength of the inducing battery is accompanied by an increase, but not by a change in the shape, of the figures. Only when they exceed a certain limit do the figures become indistinct through overlapping.

If the interruption of the current be repeated, and if in the interval an observation be made, it can be seen how, first, the number of figures increases, as by each break new ones are formed, and how, finally, by overlapping they pass into those first described.

If instead of the glass plates ebonite plates are used, the same figures appear, though somewhat larger. Also if the plate coated on one side be replaced by one coated on both sides, or by a metal plate, figures are formed on each, the more minute description of which must be reserved for another opportunity.—*Sitzungsbericht der königl. bair. Akad. der Wissenschaften*, July 1869.

#### ACTION OF SODIUM ON ACETIC ETHER.

An important step in this controversy has been taken by M. Ladenburg, who has recently sent a note to the Chemical Society of Berlin, in which he confirms Mr. Wanklyn's observation that pure acetic ether evolves *no* hydrogen when sodium acts upon it. M. Ladenburg renders acetic ether free from the last traces of alcohol by bringing it into contact with a little chloride of silicon, which attacks alcohol, but is without action on acetic ether.

#### ON THE PERIODS OF CERTAIN METEORIC RINGS.

BY DANIEL KIRKWOOD\*.

##### I. *The Meteors of April 20th.*

In the *Astronomische Nachrichten*, No. 1632, Dr. Weiss called attention to the fact that the orbit of the first comet of 1861 very nearly intersects that of the earth in longitude  $210^\circ$ , the point passed by the latter at the epoch of the April meteoric shower. A relation

\* Communicated by the Author from the Proceedings of the American Philosophical Society. Read March 4, 1870.

between the meteors and the comet, similar to that recently detected between the November meteors and the comet of 1866, was thus suggested as probable. Is this hypothesis in harmony with facts? and if not, are our present data sufficient for determining with any reasonable probability the true period of the April meteors?

*Dates of the April Shower.*—Professor Newton selects the following from Quetelet's Catalogue as belonging to this period\* :

1. B.C. 687.	4. A.D. 1093, 1094, 1095, and 1096.
2. „ 15.	5. „ 1122, 1123.
3. A.D. 582.	6. „ 1803.

*Period of the First Comet of 1861.*—The elements of this body were computed by Oppolzer, who assigned it a period of 415 $\frac{1}{4}$ . Now, while it is true that the interval from B.C. 687 to A.D. 1803 is very nearly equal to 6 periods of 415 years, the slightest examination will show that this period does not harmonize with *any of the intermediate dates*. This fact, then, without further discussion, seems fatal to the hypothesis that the period of the meteors is nearly equal to that of the comet.

*What is the probable period of the ring?*—The showers of 1093–96 and 1122–23 at once suggest a period of from 26 to 30 years. The nodal passage of the densest portion of the ring at the former epoch may be placed anywhere between 1093 and 1096, and that of the latter in either 1122 or 1123. The entire interval from B.C. 687 to A.D. 1803 is 2490 years, or 88 periods of 28 $\frac{1}{2}$ ·295 each; and the known dates are all satisfied by the following scheme:—

B.C. 687	to B.C. 15	.....	672·000	years=24	periods of 18·000 $\frac{1}{2}$	each.
„ 15	to A.D. 582	.....	597·000	„ =21	„ 28·429	„
A.D. 582	to „ 1093·714	.....	511·714	„ =18	„ 28·429	„
„ 1093·714	to „ 1122·143	.....	28·429	„ =1	„ 28·429	„
„ 1122·143	to „ 1803	.....	680·857	„ =24	„ 28·369	„

These coincidences indicate a period of about 28 $\frac{1}{3}$ † years, corresponding to an ellipse whose major axis is 18·59. Hence the distance of the aphelion is very nearly equal to the mean distance of Uranus. It will also be observed that the time of revolution, which seems to have been somewhat lengthened about the Christian era, was previously one third of the period of Uranus.

## II. *The Meteors of December 11th–13th.*

In the catalogue of Quetelet we find the four following extraordinary displays which belong undoubtedly to this period. Observations made in England, 1862, indicate also a more than ordinary number of meteors at the December epoch in that year.

1. A.D. 901. “The whole hemisphere was filled with those meteors called falling stars, the ninth of Dhu’lhajja (288th year of the Hegira) from midnight till morning, to the great surprise of the beholders in Egypt.”—Modern part of the Universal History, 8vo, vol. ii. p. 281. Lond. 1780. The date of this phenomenon corresponds to the December epoch, A.D. 901.

2. 930. “Averse remarquable d'étoiles filantes en Chine.”

3. 1571. “On vit à Zurich ‘du feu tomber du ciel.’”

\* Silliman's Journal for July 1863.

† Herrick assigned a value of 27 years. See Silliman's Journal for April 1841, p. 365.

4. 1830, 1833, and 1836. The maximum seems to have occurred in 1833, when as many as ten meteors were seen simultaneously. "Dans la nuit du 11 au 12 décembre, on vit, à Parme une grande quantité d'étoiles filantes de différentes grandeurs, qui se dirigeaient presque toutes avec une grande vitesse vers le S.S.E. A 10 heures et  $\frac{1}{4}$ , entre les seules constellations du Bélier et du Taureau, on en compta environ une dizaine."

5. (Doubtful) 1861, 1862, and 1863. Maximum probably in 1862. The meteors at this return were far from being comparable in numbers with the ancient displays. The shower, however, was distinctly observed. R. P. Greg, Esq., of Manchester, England, says the period for December 10th–12th was, in 1862, "exceedingly well defined" \*.

These dates indicate a period of about  $29\frac{1}{2}$  years. Thus :

901 to 930 . . . . .	1 period of 29·000 years.
930 to 1571 . . . . .	22 periods of 29·136 years.
1571 to 1833 . . . . .	9 periods of 29·111 years.
1833 to 1862 . . . . .	1 period of 29·000 years.

### III. *The Meteors of October 15th–21st.*

The showers of the following years (see Quetelet's Catalogue) belong to this epoch:—

1. 288. "Apparition en Chine."

2. 1436 and 1439. In each year a remarkable apparition was observed in China.

3. 1743. (Quoted from Herrick, in Silliman's Journal for April 1841.) "A clear night, great shooting of stars between 9 and 10 o'clock, all shot from S.W. to N.E. [Qu. N.E. to S.W. ?] One like a comet in the meridian very large, and like fire with a long broad train after it which lasted several minutes; after that was a train like a row of thick small stars for twenty minutes together, which dipt N."

4. 1798. "Brandès marque, à Goettingue, un grand nombre d'étoiles filantes dans les observations simultanées qu'il fait avec Benzenberg."

These dates indicate a period of about  $27\frac{1}{2}$  years:—

288 to 1439 . . . . .	42 periods of 27·405 years each.
1439 to 1743 . . . . .	11    "    27·636    "
1743 to 1798 . . . . .	2     "    27·500    "

If these periods are correct, it is a remarkable coincidence that the aphelion distances of the meteoric rings of April 18th to 20th, October 15th to 21st, November 14th, and December 11th to 13th, as well as those of the comets 1866 I and 1867 I are all nearly equal to the mean distance of Uranus.

### ON THE DIATHERMANCY OF A SERIES OF SUBSTANCES FOR OBSCURE HEAT. BY DR. SCHULTZ-SELLACK.

For the heat radiated by lampblack at 100° most substances are entirely opaque, even in layers less than 1 millim. in thickness. But a few substances are known which even in thicker layers transmit a greater proportion of this heat—namely, rock-salt, fluor-spar, sulphur, sulphate of carbon, iodine in solution, sylvine. Some of these

\* Silliman's Journal for May 1863, p. 461.

substances are elements, and some are chlorine, fluorine, and sulphur compounds. I have found that not only all *chlorides*, but also *bromides*, *iodides*, and *fluorides* of the simple substances, and also a number of *sulphides* which could be obtained in a state suitable for this investigation, have the property of transmitting a considerable proportion of the heat from lampblack. These substances have also something in common as regards their thermocroce.

The numbers in the following Table give the transmitted heat in percentages of the value of the deflections of the reflecting galvanometer produced by direct radiation.

	Transmitted heat.		
	Thickness, in millims.	Lampblack at 100°.	Gas-flame.
		per cent.	per cent.
Chloride of silver (AgCl) .....	3	46	30
Bromide of silver (AgBr).....	3	45	42
Bromide of potassium (KBr) .....	3	16	13
Iodide of potassium (KI) .....	3	11	10
Cryolite ( $\text{Al}^2 \text{Na}^6 \text{Fl}^{12}$ ) .....	10	7	23
Zincblende (ZnS) .....	5	29	23
Sulphide of arsenic with sulphur, $\text{As}^2 \text{S}^3$ ...	0.8	21	26
" " " .....	3	8	12
Vitreous selenium .....	0.4	50	36
" " .....	3	16	5

Most of the substances were not quite homogeneous; so that part of the heat must also have been lost by diffusion and by reflection.

Chloride of silver and bromide of silver are obtained, by fusion in an atmosphere of chlorine or bromine respectively, as perfectly transparent masses, which by being again fused between thin glass plates acquire smooth surfaces. The salts adhere to the glass with extraordinary tenacity; heated concentrated nitric acid, which does not perceptibly attack the salts, ascends by capillary attraction between the glass and the silver-salt and separates them. Chloride of silver appears perfectly colourless, bromide of silver of a clear amber colour.

Chloride of potassium, bromide of potassium, and iodide of potassium give, when fused, perfectly transparent masses, if the substances are used in clear crystals as obtained by evaporating their solutions at ordinary temperatures. A clear bright yellow plate of zincblende was used which Professor Rose gave me for investigation; the cryolite plate used was turbid in consequence of numerous fine cracks and slits.

Vitreous selenium is obtained in flat plates by fusing and pressing it between glass plates. Plates of less than 1 millim. thickness only transmit a narrow band in the extreme red of the solar spectrum; plates of more than 2 millims. thickness are scarcely perceptibly penetrated by intense sunlight.

Plates of mixtures of arsenic sulphide with sulphur are also obtained in the same manner. These mixtures, when they contain at most three parts of sulphur to one of sulphide of arsenic, solidify to amorphous soft masses, which only become hard and brittle after weeks.



A number of liquids and solutions of bodies which could not be obtained solid in a suitable condition, were placed in a glass vessel with sides formed of plane parallel rock-salt plates 2 millims. in thickness. The layer of liquid was 8 millims. in thickness.

The following numbers give the heat transmitted, in percentages of that which passed through the empty vessel; they thus still less give absolute values of absorption than did those in the first Table.

	Transmitted heat.	
	Lampblack at 100°.	Gas-flame.
	per cent.	per cent.
Chloride of tin ( $\text{SnCl}^4$ ).....	44	80
Chloride of sulphur ( $\text{SCI}^2$ ) .....	41	95
Bisulphide of carbon ( $\text{CS}^2$ ) .....	50	51
Phosphorus in bisulphide of carbon.....	52	57
Iodide of tin in bisulphide of carbon .....	44	47
Tetrachloride of carbon ( $\text{C}^2\text{Cl}^6$ ) in $\text{CS}^2$ ...	5	38
Chloroform, $\text{CHI}^3$ .....	2	30
Dutch liquid, $\text{C}^2\text{H}^1\text{Cl}^2$ .....	0	12
Iodide of ethyle, $\text{C}^2\text{H}^5\text{I}$ .....	0	12

The solution of phosphorus contained only 1 part of bisulphide to 10 of phosphorus; the solution of iodide of tin 1.5 part of iodide,  $\text{SnI}^4$ , to 1 part  $\text{CS}^2$ ; the solution of chloride of carbon 1 part  $\text{C}^2\text{Cl}^6$  and 4 parts  $\text{CS}^2$ . From the diathermancy of the solutions the diathermancy of the substances dissolved may be concluded.

Hence it is probable that *the haloid compounds of all elements* in a solid or liquid state *are partially diathermanous for lampblack-heat at 100° C.*; likewise many sulphides. The haloid compounds of complex radicals—of ammonium, ethyle, &c., do not seem to share the properties of the compounds of the elements.

It has been previously assumed that all substances absorb the heat of obscure sources more powerfully than those of luminous ones, excepting lampblack\*, in very thin layers, which conversely absorbs the heat of luminous sources more powerfully than those of obscure ones. In like manner, some of the substances in the first Table (selenium, zincblende, chloride of silver) exhibit the same deportment.—*Berliner Berichte*, September and October 1869.

#### ON THE INFLUENCE OF SOUNDING-VIBRATIONS ON THE MAGNETISM OF IRON. BY DR. E. WARBURG.

Matteucci†, and after him Villari‡, have examined the change which the magnetic momentum of an iron bar undergoes when it is lengthened by a pull. Villari's numerous experiments prove that the magnetism of an iron wire which is placed in a magnetizing spiral, and has attained a stable condition which is no longer altered

\* Melloni, *Ann. der Chem.* vol. lxxii. p. 40.

† *Ann. de Chim. et de Phys.* vol. lviii. p. 416.

‡ Pogg. *Ann.* vol. cxxvi.

by agitation, is altered in the opposite direction by extensions and contractions of the wire.

According to the prevalent views of the nature of magnetism, it is probable that the changes in the length and in the magnetic moment take place nearly simultaneously. If this is correct, when expansions and contractions succeed each other at very short intervals, changes in the magnetism must occur in a similar manner.

An iron bar 1890 millims. in length, which makes the longitudinal vibrations of the fundamental note, performs about 1300 entire vibrations in a second. Hence at the nodal points of the bar the transition from the greatest condensation to the greatest expansion takes place in the  $\frac{1}{1300}$  of a second.

The author has found in fact that, provided the iron is sufficiently soft, even in this case, in spite of the smallness of the elongations and notwithstanding the rapidity of the oscillations, very appreciable variations in the magnetic moment accompany the periodical changes in density.

A change in the magnetism is most easily proved by the induction-currents which it produces. But if equal and opposite changes in the magnetism succeed each other, alternating induction-currents are produced which cannot be detected by the ordinary galvanometers. Yet W. Weber was able to demonstrate the existence of such alternating currents by the electro-dynamometer, which is indifferent to the direction of the currents which traverse it. W. Weber used the above instrument \*, which is of his invention, to demonstrate the alternating induction-currents which are produced in an induction-spiral by the *periodical motions* of the free ends of a transversely sounding magnetic steel bar. I myself have used the same instrument to demonstrate the induction-currents which are produced in an induction-spiral by the *periodical changes* of the magnetic moment on the nodal point of an iron wire surrounded by a magnetizing spiral.

A piece of commercial iron wire 1890 millims. in length was firmly fastened in the middle, so that, when rubbed with a piece of leather coated with powdered resin, it produced the longitudinal fundamental note, and thus a nodal point was formed in the middle. One half of this wire was almost entirely surrounded by a magnetizing spiral; on the other half, at the node, was a short induction-spiral of copper wire, while the rest was free, so that the wire could be rubbed. The short induction-spiral was inserted into a circuit containing a reflecting dynamometer, and the motions of the mirror were observed by means of a scale and a telescope. The magnetizing current was furnished by two Bunsen's elements. When the rod was strongly rubbed, a deflection was obtained in the galvanometer of from thirty to fifty divisions. When the induction-spiral was pushed on the free end of that part of the wire which was left uncovered by the magnetizing spiral, no deflection was obtained in the galvanometer. It must thence be concluded that the induction-currents in the first case are not produced by the backward and forward gliding of the magnetic particles of iron, which gliding is least in the middle (that is, at the node) and is most powerful at the

\* *Electrodyn. Maassbest.* vol. i. § 16.

free ends of the loops, but is due to alterations of the magnetism, owing to the alternate condensations and expansions, which are greatest at the nodes and are null at the free ends.

When the dynamometer was replaced by a galvanometer, the needle of the latter exhibited no deflection, but only irregular movements. These arose from the variations of the magnetic iron wire inseparably connected with the rubbing; for those actions took place if the variations of the wire were produced without any note. It is therefore to be concluded that the action upon the dynamometer is to be ascribed to alternate opposite induction-currents of equal strength, which indicate alternately opposite equal changes in the magnetism.

Of other commercial iron wires, of similar dimensions to that of the wire previously used, two did not at all exhibit the phenomena described on sounding, while another did so, but extremely feeble. These wires were now heated in the middle, where a node was formed when the fundamental note was sounded, and the experiment was repeated. All three iron wires now exhibited the phenomenon in question so powerfully as to produce a deflection of 200–300 divisions of the dynamometer. The wire used in the first experiments, when heated at the node, gave also, when powerfully rubbed, a deflection of 500–600 divisions. A steel wire, on the contrary, after strong heating did not exhibit the phenomena in question. Matteucci states, in accordance with these results, that the magnetic moment of soft iron wires is more strongly changed by drawing than is that of hard wires.

To judge the strength of the effect obtained, it may be stated for instance, that in one experiment, on opening the magnetizing spiral (that is, by the almost total disappearance of the entire magnetism of the iron), a current was induced in the induction-spiral which only produced a deflection of three divisions in the dynamometer; while the action of the currents produced by continuous sounding in the induction-spiral was so far accumulated in the dynamometer that the bifilar coil produced a deflection of an area of 460 divisions.

An increase in the magnetism of the soft-iron wire, whether by an increase of the current-producing elements or by using several magnetizing spirals, gave no corresponding increase in the magnetic variations on sounding. Just in like manner, in Villari's experiments a strengthening of the magnetizing current was not always succeeded by a stronger change of the magnetic moment in consequence of traction; and even when it was, only in the case of certain wires.

When instead of two elements only one was used, feebler actions were obtained; but even when the magnetizing current was opened the remanent magnetism of the soft iron wire was sufficient to produce a deflection of from fifty to sixty divisions in the dynamometer when the bar was made to sound.

Considerably smaller variations of the magnetic moment were indicated by the dynamometer in a node of the first upper tone of the bar of 2600 vibrations in a second.—*Monatsbericht der Berliner Akademie*, December 1869.

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AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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JUNE 1870.

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LVIII. *A Theory of Nebulæ and Comets.* By A. S. DAVIS, B.A.,  
*Mathematical Master, Leeds Grammar School.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

**W**ILL you allow me to lay before your readers a theory regarding nebulæ and comets which I believe is in a great degree new, and which appears to me to afford a satisfactory explanation of the phenomena observed in those remarkable bodies?

The theory may be stated as follows:—There exist in space large masses of matter in a gaseous, non-luminous, and therefore invisible state. This matter is necessarily very rare, because there is no solid nucleus to condense it by its attraction. As long as a mass of such matter remains by itself, it continues gaseous, non-luminous, and invisible. It will, however, sometimes happen that two masses of gas having a chemical affinity for each other rush together under the influence of their mutual gravitation. When this occurs, chemical combination will take place, with a consequent evolution of light and heat, and a nebula will begin to be formed. Chemical action will only take place where the gases become mixed—that is, about their common bounding surfaces; and the greater part of the matter will remain invisible. The apparent shape, then, of a nebula will by no means indicate the real shape of all the matter composing it, but will rather reveal the form of the common bounding surfaces of the different masses of gas.

The gases, so long as they remain separate, cannot condense into a liquid or solid state; but the compound formed by the  
*Phil. Mag.* S. 4. Vol. 39. No. 263. June 1870. 2 D



chemical union may only be able to exist as a gas so long as it remains at a great heat. After the compound has formed, it will cool down, and may ultimately assume the liquid or solid state.

When liquid or solid matter is formed, it will gradually aggregate and gravitate towards the centre of the nebula, and will form into a nucleus there. It is probable that chemical action will not be the only source of light and heat, but that they will also be produced by the loss of motion of the solid and liquid parts moving through the gases.


This theory attempts to explain how nebulæ originally came into existence and assumed the shapes they actually have. And in this it differs from that of Sir William Herschel, who only speculated on what would be the subsequent motions and behaviour of nebulous matter after it had once been formed.

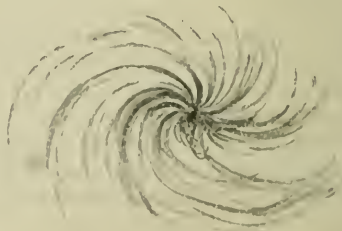
I will now proceed to examine what account this theory renders of the phenomena observed in nebulæ.

And first, I think, if we admit that a great part of the matter forming a nebula is invisible, we remove the immense difficulty which we otherwise experience in attempting to conceive how nebulæ have assumed or can retain their extraordinary shapes and yet be subject to the law of gravitation.

The researches of Huggins prove that many of the nebulæ are gaseous self-luminous masses, whilst the spectra of others prove the existence of solid or liquid matter. These last, being generally those which are most easily resolved, would be those which have been longest in existence, and which, therefore, should contain most liquid and solid matter.

The curious shapes of the nebulæ are well explained on this theory.

Thus the spiral nebulæ would be produced by two masses of gas rushing together and forming an eddy in which they become mingled with each other. If any detached portion of one gas got mixed up with the other gas, it would be drawn out by the eddy into an elongated spiral form, the surface of which would become visible by the light produced by the chemical action. This  would be the kind of nebula which would be produced when the two masses of gas did not differ greatly in size, and rushed together in a line not very oblique to the line joining their centres of gravity.



Some spiral nebulæ consist only of a single whirl. A nebula

of this kind might be produced by a small mass of comparatively dense gas rushing obliquely into a large mass of rarer gas\*. Such a mass would tend to the centre of gravity of the larger mass; and since those parts which are nearest the centre of gravity would be most accelerated, it would become drawn out into an elongated spiral form.

After a time the whole of the smaller mass of gas will have collected about the centre, and a globular or planetary nebula will be formed.

In the annexed diagram, copied from a drawing by Lassell, we appear to have a globular nebula in the process of formation. That the smaller mass of gas has entered obliquely is shown by the fact that the ending, *ab*, of the tubular portion is oblique to the axis of the tube; for the line *ab* will indicate the boundary of the larger mass. That the gas is collecting about the centre of the larger mass is indicated by the fact that the line *oc* joining the centre of the globe with the end of the tube is perpendicular to *ab*—as it should be, supposing the larger mass of a spherical form.

Planetary nebulæ consisting of more than one envelope might also be formed in the same way as those having only one envelope, the envelopes being formed in succession at different periods of time.

Annular nebulæ might be formed in the following manner. If a small mass of gas were attracted to a larger mass so as just to graze its surface on passing it, its path (previously parabolic) would become elliptic, and its motion would be changed into a periodical revolution round the larger



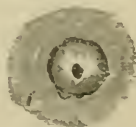
Lassell, pl. 1. fig. 3†.



Lassell, pl. 7. fig. 30.



Lassell, pl. 2. fig. 9.



Lassell, pl. 1. fig. 4.

\* When I speak of one gas as being rarer than another, I mean that, when subject to equal pressures, the one gas is specifically lighter than the other.

† See Memoirs of the Royal Astronomical Society, vol. xxxvi.

mass. At each revolution it would graze the surface of the larger mass, and the elliptic orbit would gradually be changed into a circular one. It would at length come to revolve about the larger mass in such a manner as continually to graze the surface, and an annular nebula would be the result.



Nebula in Lyra.

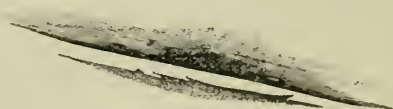
Those nebulæ, like the large one in Andromeda, in which streaks of darkness cross the bright parts, offer the greatest difficulty in their explanation.

We might imagine that a mass of rare gas has become entangled between two masses of dense gas, and that the masses of dense gas, being attracted to one another, crush out the rare gas into a thin sheet, which, looked at edgewise, presents the appearance of a dark streak.

Some of the nebulæ of this kind have nuclei; and it may be remarked that the position of the nucleus, which will be the centre of gravity of the nebula, is in the position in which we should expect to see it if the apparently vacuous streaks were in reality filled with matter.



Lassell, pl. 5. fig. 21 A.



Lassell, pl. 5. fig. 21.

Many of the nebulæ are very much diffused, and in shape very irregular; but even these show certain characteristics which seem to indicate their mode of formation. The extreme faintness of their light favours the hypothesis that they are not luminous throughout, but that their luminosity occurs only here and there in comparatively thin shells.

Again, if these nebulæ were luminous throughout, the intensity of their light should be greatest where the thickness of matter looked through is greatest, and should fade away at the boundaries, whereas, if the visible parts occurred in shells, the boundaries should appear brighter than other parts; for at the boundaries the line of sight would be very oblique to the shell, and a greater thickness of luminous matter would be looked through. Now if we examine any of the irregular nebulæ, we see some parts of it ending abruptly with a clearly defined out-

line, whilst in other parts the light gradually fades away into darkness. We may suppose that in the one case we are looking edgewise at a shell, so that the line of sight is tangential to its surface, whilst in the other case we are looking at a part where the luminous shell gradually thins out. Now in the first case we find that the light increases towards the border, consistently with the supposition that the visible parts are in thin shells.

This peculiarity is, I apprehend, what Professor Bond and Mr. Lassell refer to when they speak of the scroll-like appearance of the nebula in Orion.

In those nebulæ which appear to have assumed a more stable form, as in the elliptic and circular nebulæ, the light diminishes from the centre to the circumference.

These nebulæ may be in a later stage of formation; they may have become more condensed, and a more intimate mixture of the gases may have taken place, and in consequence they may be more or less luminous throughout. Their shape, and the fact that they are generally resolvable in the telescope, point to the conclusion that they have been a long time in existence.

Before turning to the subject of comets, I may remark that the theory that there are large masses of invisible gas traversing space, affords an explanation of the sudden temporary outbursts in the intensity of certain stars. Such an outburst might occur if a star became enveloped with a mass of gas with which it could chemically combine.

It also explains the phenomena of periodical stars. Mr. Huggins has found that the diminution in the intensity of the light of these stars is due to an increase in the number and size of the absorption-bands in their spectra.

It has already been suggested that large opaque bodies may be revolving in orbits round such stars and periodically obscuring their light. We have only to suppose that large masses of invisible gas instead of opaque bodies revolve about them, to explain the phenomenon in accordance with spectroscopic observation.

I will now examine what account the present theory gives of comets.

There are some reasons for supposing that the sun itself is a nebulous star, or that it is enveloped by matter extending to an immense distance beyond its visible photosphere. The phenomenon of the zodiacal light, the bands of meteoric matter passed through by the earth in its revolution about the sun, the retardation experienced by comets, all point to this conclusion.

We cannot suppose that this envelope consists of solid or liquid matter only, without the presence of gaseous matter; for at no known temperature can liquid or solid matter exist in a



vacuum without evaporation. If, then, we suppose for a moment that solid matter exists without being enveloped by gas, it will immediately begin to evaporate and form an atmosphere about itself. This atmosphere will be very rare and very extensive, as the central mass, being comparatively very small, will exert but a feeble attraction on it.

Or, again, if we suppose that meteoric matter unenveloped by gas would not acquire an atmosphere by evaporation, it would do so in another manner; for it is certain that some of the meteoric bands approach very near to the sun in their perihelion. These would attract to themselves a part of the sun's atmosphere, which they would carry away with them on their departure; and portions of this they would, in their turn, part with to every meteor which came within a sufficiently small distance from them.

Graham has found that meteoric matter which has fallen to the earth gives evidence of having been exposed, when at a high temperature, to hydrogen existing under a pressure of several atmospheres.

We conclude, then, that the sun is surrounded by an envelope of gas, which is not a true solar atmosphere, but is the aggregation of the atmospheres of numberless meteoric bodies revolving around it.

Now M. Hoek has shown that comets are detached portions of large masses of matter; and it has been suggested that these large masses may be nebulæ. Admitting this, a comet, before its entrance into the solar system, will consist of a solid or liquid nucleus surrounded by a large mass of very rare invisible gas. On its approach to the sun, the nucleus will make its way most easily into the solar envelope, and the comet will enter with its tail directed away from the sun. A chemical combination will take place between the tail of the comet and some of the gaseous elements of the solar envelope; and where this combination occurs, the gases will become visible from the light evolved, and, if the compound formed be in a solid or liquid state, from the light also which it reflects from the sun—or if, as probably would be the case, the matter be in a state of minute subdivision, from light scattered by that kind of dispersion which Professor Tyndall has lately shown is produced by finely divided matter.

On passing through its perihelion, the comet loses a great part of its tail, which soon cools down and becomes mingled with the rest of the solar envelope.

On leaving the sun, the tail begins to increase, from the addition to it of matter rendered gaseous by the heat of the sun. Those parts of the gas where chemical action has taken place being heated, and therefore rendered specifically lighter than

the unheated invisible gases, will have a tendency to escape out of the solar envelope in addition to that which they possess from their momentum in common with the rest of the comet. Hence the comet will depart with its tail directed away from the sun.

The hollow appearance of many comets, and the isolation of the nucleus from the vertex of the coma, are in accordance with this theory.

On a comet's approach to the sun, it often happens that a tail of immense length is formed in a very few days. It is usually supposed that the matter forming the tail has all been projected from the head within the time of its first becoming visible, and consequently that it has moved with enormous velocity in a direction opposed to the sun's attraction. Hence it has been conjectured that the matter forming the tail is not subject to the same mechanical laws as those which govern all other known matter.

On the present hypothesis there is no need to assume this enormous velocity. A comet, in fact, enters the solar envelope with a tail of invisible gas. It may be that chemical union cannot take place between this gas and the sun's envelope until the heat of the sun, acting on the head of the tail, has set up chemical action or combustion—until, in short, the comet has been lit by the sun's heat. When once combustion has commenced, it would spread into the tail with prodigious velocity.

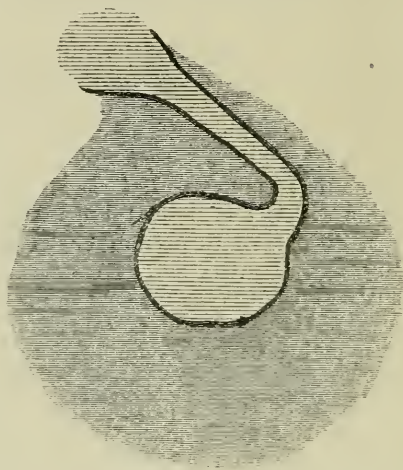
Tails of comets have been observed to form with enormous speed only on their approach towards the sun. The tails which form when a comet is receding from the sun are produced with comparative slowness: this we should expect; for in this case there is not already in existence a tail needing simply to be lit to become visible.

The more any portion of the gas of a comet becomes removed from the nucleus, the greater will be the volume it occupies, because of the diminution in the pressure due to a diminution in the attractive force of the nucleus. This will account for the spreading shape of the tail of a comet.

We may explain in a similar way the increase in the size of a comet as it recedes from the sun; for the pressure of the solar envelope upon it will become less as its distance from the sun becomes greater. M. Valz has attempted an explanation of this fact in a somewhat similar manner. He conceives that the increase in the size is due to a diminution in the pressure of the ætherial medium, which he supposes to be denser in the neighbourhood of the sun than elsewhere. Sir J. Herschel objects to this explanation, on the ground that we must suppose that the æther does not pervade the matter composing the comet.

This objection does not apply to the present explanation.

The following figure is intended to represent a section of the nebula (Lassell, pl. 2. fig. 9, Royal Astronomical Society's Memoirs, vol. xxxvi.) shown on page 403.



The parts represented by the medium shading and the light shading denote, respectively, the large mass of rarer gas and the small mass of denser gas. The dark shading denotes the part rendered visible by chemical action.

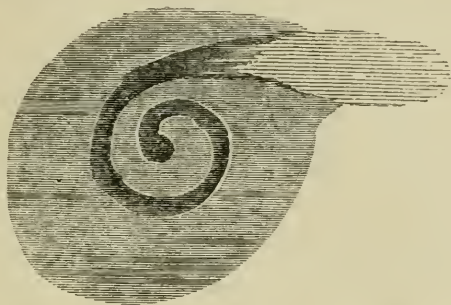
Immediately below is shown the mode of formation of an



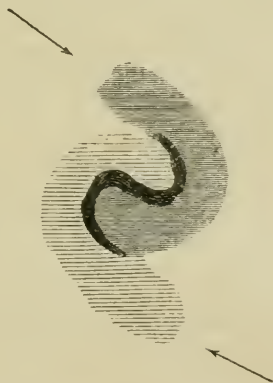
annular nebula. The mass of gas indicated by the medium shading must be smaller than that indicated by the light shading, and also rarer. If it were denser, it would ultimately enter into

it, and a spiral nebula would be formed. The dark shading, as before, shows the visible part.

This represents the formation of a spiral nebula:—



Lassell gives several drawings of S-shaped nebulae having nuclei in the middle.



The above diagram is intended to represent the mode of formation of a nebula of this kind.

Two masses approaching each other move in parabolas, having their centre of gravity as a common focus. On approaching each other they are drawn out into an elongated form; and if a collision occurs, the common boundary will be shaped as in the figure.

ARTHUR SLADEN DAVIS.

Roundhay Vicarage,  
March 18, 1870.



LIX. *Thermo-chemical Researches.*

By M. JULIUS THOMSEN\*.

PART I.—Berthollet's *Theory of Affinity*.

IN his Essay on Chemical Statics, Berthollet laid it down as a rule of the action of acids on salts, that a base when in presence of two acids tends to divide itself between them in proportion to the bulks of those acids and their respective affinities for that base. But up to the present time no one has been able to submit this theoretical law to a satisfactory experimental verification. M. Thomsen thought to accomplish this by thermo-chemical researches. For since different acids disengage different quantities of heat in combining with the same base, the decomposition of a salt by an acid must be accompanied by a thermic effect†, which will be greater the more complete the decomposition, and which ought to admit of measurement.

The aim of the present memoir is the special study of the reactions of nitric acid and hydrochloric acid on sulphate of soda; but each of them requires a very great number of experimental determinations. Thus, for the first we must know the thermic effect produced in the following reactions:—

1. Neutralization of sulphuric acid by soda.
2. Neutralization of nitric acid by soda.
3. Decomposition of sulphate of soda by nitric acid.
4. Decomposition of nitrate of soda by sulphuric acid.
5. Supersaturation of sulphate of soda by sulphuric acid.
6. Supersaturation of nitrate of soda by nitric acid.
7. Action of sulphuric acid on nitric acid.

Reactions 3, 4, 5, and 6 must, moreover, be studied with varying proportions of the substances concerned in the action.

The author observes that, in order to derive any advantage from these data, they must be known with great accuracy. He thinks it is necessary that the approximation should be at least to within  $\frac{1}{2}$  per cent.; and he believes that this was attained in his determinations. He adds that he could use no previous determinations, because they were far from approaching this degree of precision. Thus the heats determined by Messrs. Favre and Silbermann for the neutralization of nitric, hydrochloric, hydrobromic, and hydriodic acids are from 10 to 12 per cent.

\* Translated from an abstract of an article in Poggendorff's *Annalen* (vol. cxxxviii. p. 65), published by M. C. Marignac in the *Bibliothèque Universelle de Genève*, December 1869, pp. 301–318.

† [The author has coined a new word for this case, *Wärmetönung*, which ought perhaps to be translated *calorific tonality*, but for which we think the more simple expression, *thermic effect*, may be used, which can alike denote an absorption or a disengagement of heat.—C. M.]

too high, whilst that which they have obtained for sulphuric acid is nearly exact.

The experimental method employed was that of mixtures, the calorimeter being carefully preserved from all external radiation. The temperatures were determined by means of thermometers calibrated and compared with the greatest care, and on which the  $\frac{1}{200}$  of a degree could be read off with certainty. The weights of the liquid mixed were usually 450 grammes of each of the solutions. In the majority of cases these solutions contained 200 equivalents of water to one of the reacting body; sometimes, however, solutions two or three times as dilute had to be employed.

For solutions thus dilute we can, without fear of an appreciable error in the results, calculate the thermic effects by merely taking into consideration the weight of the water. Moreover, as the temperature soon acquires a state of equilibrium (in less than a minute), whenever the variation in temperature does not exceed a degree, it can be proved that it remains for several minutes without appreciable change, so that there is no occasion for any correction in the result obtained. In the few cases in which this variation is more considerable, the thermometer must be read every minute from the moment at which the mixture was made; then it can be calculated what the maximum temperature must have been, independently of external influences.

The value in water of the vessel used as the calorimeter, and of the thermometer, had been determined by experiment; it was either 13 grammes or 9.7, according as one or the other of the two vessels intended for these experiments was used.

The exactitude of the method was established by numerous experiments, in which water of different temperatures was mixed. The maxima of error in forty-two experiments were +7 and -8 thermal units. In a series of eighty-eight comparative experiments, the departure from the mean only once reached 9 thermal units, and in eighty-four of these series it was less than 5. Hence we may assume that the possible maximum of error in each determination does not exceed  $\pm 5$  thermal units; and as the quantity of matter employed in each of them represents in general from  $\frac{1}{4}$  to  $\frac{1}{6}$  of an equivalent, the limit of the maximum of error must be  $\pm 20$  or  $\pm 30$  thermal units for the thermic effects proportional to an equivalent of the substance (the equivalent of hydrogen being taken as unity).

The following are the results of these determinations, of which we only give a summary.

*Neutralization of soda by sulphuric acid, nitric acid, and hydrochloric acid.*—Both the base and the acid were in every case dissolved in 200 equivalents of water. The heats disengaged for

one equivalent of the base and acid were\* :—

	Thomsen.	Favre and Silbermann.
Sulphuric acid . . .	15689	15810
Nitric acid . . .	13617	15283
Hydrochloric acid . .	13740	15128

*Supersaturation of soda by sulphuric acid.*—One equivalent of sulphate of soda gives rise, by the addition of sulphuric acid, to a very remarkable absorption of heat, which increases with the proportion of the acid, as the following numbers show :—

	Thermal units.
For $\frac{1}{4}$ equivalent of acid . . .	— 396
$\frac{1}{2}$ „ . . .	— 631
1 „ . . .	— 935
2 „ . . .	— 1176
4 „ . . .	— 1341

For intermediate proportions, the thermic effect may be calculated with sufficient approximation by the formula

$$-\frac{n}{n+0.8} 1650,$$

$n$  denoting the number of equivalents of acid added to one equivalent of sulphate of soda.

*Supersaturation of soda by nitric acid and by hydrochloric acid.*—These reactions occasion a scarcely perceptible absorption of heat. We have found, for one equivalent of nitrate of soda and one equivalent of nitric acid, —36 thermal units; for one equivalent of chloride of sodium and one equivalent of hydrochloric acid, —32 thermal units. Hence, in calculating complicated reactions, we might neglect the thermic effects due to these causes; and so it appeared useless to repeat these determinations for other relative proportions of acid and salt.

*Decomposition of sulphate of soda by nitric acid.*—We first studied the decomposition of neutral sulphate of soda by various quantities of nitric acid. The sulphate was always dissolved in 200 equivalents of water; the nitric acid varied from  $\text{NO}^5 + 100$  aq to  $\text{NO}^5 + 300$  aq. Experiment had shown that the addition of 100 equivalents of water to 1 of nitric acid which already contained 100 equivalents of it, disengages so small a quantity of heat that it may be completely neglected†. For one equivalent

\* [We have placed the results obtained by the author side by side with those obtained by Messrs. Favre and Silbermann.]

† This remark of the author's seems to indicate that he attributes no influence to the degree of dilution of the solutions, except in proportion to the thermic effect developed in their preparation. That would be, we think, to neglect the most important cause of the variations which that di-

of sulphate of soda we obtained—

			Thermal units.
with $\frac{1}{8}$	equivalent of nitric acid	. .	— 452
$\frac{1}{4}$	”	”	— 808
$\frac{1}{2}$	”	”	— 1292
1	”	”	— 1752
2	”	”	— 2026
3	”	”	— 2050

It may be concluded from this that the decomposition increases with the proportion of nitric acid. The complete transformation of sulphate of soda into nitrate would give rise to an absorption of 2072 thermal units.

In a second series of experiments, the action of two equivalents of nitric acid on one equivalent of acid sulphate of soda of various degrees of acidity,  $\text{NaO}, (1 + \gamma) \text{SO}^3$ , was examined; we have for

	Thermal units.
$\gamma = 0$ . . . .	— 2026
$= 1$ . . . .	— 978
$= 2$ . . . .	— 664
$= 3$ . . . .	— 520

In order to appreciate the influence which the presence of pre-existing nitrate of soda could have on the reaction, nitric acid was made to act on a mixture of sulphate and nitrate of soda in equal quantities,



	Thermal units.
we have for $\beta = \frac{1}{4}$ . . . .	— 546
$= \frac{1}{2}$ . . . .	— 761
$= 1$ . . . .	— 968

*Decomposition of sulphate of soda by hydrochloric acid.*— We have only examined the action of various proportions of hydrochloric acid on an equivalent of neutral sulphate of soda. The results are:—

	Thermal units.
For $\frac{1}{2}$ equivalent of acid . .	— 1247
1 ” . . . .	— 1682
2 ” . . . .	— 1878
4 ” . . . .	— 1896

Thus with hydrochloric acid, and likewise with nitric acid, the

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lution may occasion in the thermic effects accompanying the double decompositions. We shall return to this subject in the following article (*Bibl. Univ.* p. 319).—C. M.]



sulphate of soda experiences a decomposition increasing with the proportion of acid. Its complete conversion into chloride would absorb 1949 thermal units.

*Decomposition of nitrate of soda by sulphuric acid.*—This reaction gives rise to a disengagement of heat which, for an equivalent of nitrate of soda, rises to—

	Thermal units.
For 1 equivalent of sulphuric acid . .	288
2 equivalents of sulphuric acid . .	379

*Decomposition of chloride of sodium by sulphuric acid.*—Analogous results :—

	Thermal units.
For 1 equivalent of sulphuric acid . .	244
2 equivalents of sulphuric acid . .	336

*Discussion of the Results of these Experiments.*

It follows from the preceding experiments, that when nitric acid or hydrochloric acid acts on sulphate of soda, they give rise to an absorption of heat, whilst the reaction of sulphuric acid on the nitrate or chloride disengages heat.

It is evident that the thermic effect which accompanies these reactions may be regarded as the measure of the chemical decomposition; and experiment confirms this notion. When an equivalent of sulphuric acid acts on an equivalent of nitrate of soda, the same product is obtained as when an equivalent of nitric acid acts on an equivalent of sulphate, or as when an equivalent of each of the two acids acts on an equivalent of soda simultaneously.

Now it follows, from the thermochemical principles previously explained by the author and admitted by all without dispute, that the heat disengaged by the reaction of three bodies (A, B, and A') is the same, whatever may be the order in which they react :

$$(A, B, A') = (A, B) + (AB, A') = (A', B) + (A' B, A) ;$$

from which it follows that

$$(AB, A') - (A' B, A) = (A', B) - (A, B) ;$$

which, in the present case, may be interpreted thus : *the difference between the thermic effects produced by the reaction of sulphuric acid on nitrate of soda, and by that of nitric acid on the sulphate, is equal to the difference of the heats disengaged in the neutralization of soda by the two acids.*

In fact, experiment gave for these differences :—

	Thermal units.
15689	— 13617 = 2072
288	— (— 1752) = 2040.

The difference of the two results is 32 thermal units, or 2 for every thousand of the heat of neutralization; now the exactitude of experiments cannot attain a closer approximation. In the case in which hydrochloric acid replaces nitric acid, we have

Thermal units.

$$15689 - 13740 = 1949$$

$$244 - (-1682) = 1926,$$

the agreement being as satisfactory as possible.

We shall now show how to calculate the magnitude of the decomposition for the reaction of an equivalent of nitric acid on an equivalent of sulphate of soda.

Let B be the soda, A the sulphuric acid, A' the nitric acid, and  $x$  the proportion of decomposed sulphate of soda; we shall have in the liquid after the reaction,

$$(1-x)BA + xBA' + xA + (1-x)A'.$$

We may regard the total reaction, and the thermic effect which accompanies it, as the result of a series of simple reactions, of which the thermic effects have been previously established; that is to say:—

1. Decomposition of  $x$  equivalents of the salt BA,  $-x$  15689 thermal units.

2. Formation of  $x$  equivalents of the salt BA',  $+x$  13617.

3. Action of  $(1-x)$  equivalents of the sulphate of soda on  $x$  equivalents of sulphuric acid, say

$$(1-x) \left( \text{NaO SO}^3, \frac{x}{1-x} \text{SO}^3 \right).$$

4. Action of  $x$  equivalents of nitrate of soda on  $1-x$  equivalents of nitric acid: we have seen that the thermic effect produced in this reaction may be neglected.

5. Action of  $x$  equivalents of sulphuric acid on  $(1-x)$  equivalents of nitric acid: experiment proves that the thermic effect produced by this reaction is not measurable.

The sum of these effects must be equal to the total thermic effect shown in the reaction; hence

$$-x 2072 + (1-x) \left( \text{NaO SO}^3, \frac{x}{1-x} \text{SO}^3 \right) = -1752 \text{ ther. units.}$$

We may deduce the value of  $x$  from this empirically, by utilizing the previously cited experiments which give the thermic effect resulting from the action of sulphuric acid in various proportions on sulphate of soda. We thus find that the preceding formula is very exactly satisfied by the value  $x = \frac{2}{3}$ , because we have then, for

the value of the left-hand member,

$$-\frac{2}{3}2072 + \frac{1}{3}(\text{NaO SO}^3, 2\text{SO}^3) = -\frac{2}{3}2072 - \frac{1}{3}1176 = -1772$$

thermal units, a result which agrees within 20 thermal units with the result of the experiment.

By some analogous calculations we find that the thermic effect produced in the inverse reaction of sulphuric acid on nitrate of soda would be expressed by the formula

$$(1-x)2072 + (1-x)\left(\text{NaO SO}^3, \frac{x}{4-x}\text{SO}^3\right) = 299 \text{ thermal units,}$$

whilst the experiment gave 288 thermal units. Hence it follows from these experiments that:—

(a) When an equal number of equivalents of soda, sulphuric acid, and nitric acid are placed together in the same solution, two-thirds of the soda enter into combination with the nitric acid and one-third with the sulphuric acid.

(b) The division of the base between the two acids occurs in the same manner, whether it has been introduced in the state of sulphate or in that of nitrate.

(c) Hence nitric acid has twice the tendency to combine with the base which sulphuric acid has; it is therefore remarkably more energetic in the moist way.

It is necessary for the development of these phenomena to employ a term in order to express this tendency of acids, more or less great, to neutralization. It cannot be designated by the word *affinity*; for this term must be reserved, according to the author, to express the force which must be overcome in order to decompose a combination into its elements, and which can be measured by the work necessary for this decomposition (therefore by the thermic effect). The phenomena in question are altogether of a different nature; and M. Thomsen proposes the word *avidity* to denote this tendency of acids to neutralization. Thus the *avidity of nitric acid for soda is twice that of sulphuric acid*.

Some calculations precisely similar show that it is the same for hydrochloric acid: for soda its avidity is double that of sulphuric acid.

In this hypothesis, in fact, we find that the decomposition of an equivalent of sulphate of soda by an equivalent of hydrochloric acid must absorb 1691 thermal units; experiment gave 1682. The inverse reaction of sulphuric acid on chloride of sodium must disengage heat, 258 thermal units; experiment gave 244\*.

\* [It might at first sight seem strange that the avidity of acids for bases varies inversely as their affinity (the latter being measured by the heat disengaged in neutralization), if we did not remark that the author has here

The author has made some analogous experiments with a great number of acids. In none of those which he has examined is the avidity equal to that of nitric acid and hydrochloric acid; but it is in some superior, in others inferior to that of sulphuric acid; in some it is almost nothing. He announces the forthcoming publication of these results, and passes now to the comparison of his observations with Berthollet's law.

According to Berthollet, the division of a base between two acids takes place in proportion to its affinity for those acids and to their respective masses. Now the masses are proportional to the chemical equivalents  $A$  and  $A'$  of the acids, and to the numbers  $a$  and  $a'$  of equivalents of each of them which are placed in the presence of the base. Therefore if we denote their respective affinities for that base by  $x$  and  $x'$ , we shall have for the ratio according to which the division of the base is effected:—

$$r = \frac{aAx}{a'A'x'}.$$

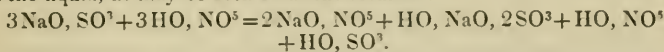
But that which Berthollet designated the affinity of the acids was no other than that which was called their capacity of saturation—that is to say, the inverse of their chemical equivalents; so that this formula becomes simply

$$r = \frac{a}{a'};$$

and the base ought to divide itself between two acids in proportion to the number of equivalents of the latter.

Hence, in the presence of equal equivalents of sulphuric acid and nitric acid, soda ought to divide itself equally between them. Therefore Berthollet's law is not confirmed by experiment. The majority of modern authors have interpreted the law of Berthollet in a sense less conformable to its strict expression, but more in harmony with the ideas which have given rise to the theory of chemical equivalents, by attributing to the word affinity a sense analogous to that which its author attributes to the term avidity,

compared a bibasic acid with two monobasic acids, so that, the relative energies of these acids not being very different, the division occurs in such a manner as to produce a neutral nitrate and a bisulphate—equivalent quantities of sulphuric acid and nitric acid or hydrochloric acid remaining free in the liquid, as may be seen from the formula:—



If we assume that this is the principal cause of the division which the base undergoes, it remains to be known whether the influence of the degree of energy of the acids has not the effect of slightly modifying this state of equilibrium. But in order to be able to verify this supposition, it would be necessary to attribute to the experimental data a precision of which they do not appear susceptible, for reasons which I shall hereafter explain.—C.M.]



and by saying that the division of the base takes place in proportion to the number of equivalents of the acids and to their energy,

$$r = \frac{am}{a'm'},$$

$m$  and  $m'$  expressing the affinities or energies of the acids for the base.

This formula would well apply to the reaction of an equivalent of nitric acid on an equivalent of soda, on the supposition that  $\frac{m}{m'} = \frac{1}{2}$ . But if we try to calculate with the same ratio the other reactions, in which various quantities of nitric acid are put into the presence of an equivalent of sulphate of soda, and if we then calculate the thermic effects corresponding to these various reactions, we obtain results which in no way accord with those furnished by experiment\*.

Thus Berthollet's theory does not accord with experiment even in the form under which it is usually presented. A formula very different from that of Berthollet has been proposed by M. Guldberg†.

Let A and B be two bodies which by their reaction produce the bodies C and D (thus, in the present case, A would be sulphate of soda, B nitric acid, C nitrate of soda, D sulphuric acid); let  $\alpha$ ,  $\beta$ ,  $\gamma$ , and  $\delta$  be the number of equivalents of these four bodies respectively which are placed together, and  $x$  the number of equivalents of A and of B which mutually decompose one another, so that after the reaction we have

$$(\alpha - x)A + (\beta - x)B + (\gamma + x)C + (\delta + x)D.$$

If we denote by  $k$  the energy with which the reaction of A upon B takes place, and the inverse reaction of C upon D by  $\frac{1}{k'}$ , we shall have, according to the theory of Guldberg,

$$k(\alpha - x)(\beta - x) = \frac{1}{k'}(\gamma + x)(\delta + x).$$

If the product  $kk'$  be expressed by  $n^2$ , we shall have

$$x = \frac{n^2(\alpha + \beta) + \gamma + \delta - \sqrt{[n^2(\alpha + \beta) + \gamma + \delta]^2 - 4(n^2 - 1)(n^2\alpha\beta - \gamma\delta)}}{2(n^2 - 1)}.$$

In order to calculate the value of  $x$  by means of this formula (that is, the magnitude of the decomposition), we must determine the value of  $n^2$  by means of the reaction in the most simple case, when an equivalent of nitric acid acts on an equivalent of sul-

\* We omit here the details of these calculations and the Tables of comparison of the results, and only give the conclusions.

† *Etudes sur les affinités chimiques*, par G. M. Guldberg et P. Waage. Christiania, 1867.

phate of soda. We then obtain

$$\alpha = \beta = 1, \quad \gamma = \delta = 0;$$

so that the value of  $x$  is simply

$$x = \frac{n}{n+1}.$$

Now, experiment having shown that in this case  $x = \frac{2}{3}$ , it follows that  $n = 2$ .

Thus, for all the reactions which take place between sulphuric acid, nitric acid, and their salts of soda, we shall have

$$x = \frac{1}{6} [4(\alpha + \beta) + \gamma + \delta - \sqrt{[(\alpha + \beta) + \gamma + \delta]^2 - 12(4\alpha\beta - \gamma\delta)}].$$

M. Thomsen has calculated, by means of this formula, the magnitude of the decomposition in all the experiments in which he caused nitric acid to act in various proportions on combinations equally varied of soda and sulphuric acid, and afterwards the thermic effects corresponding to the decompositions indicated by this theory.

The following Table recapitulates all these results, as well as those which relate to the action of hydrochloric acid, and which have been calculated by the same formula. In it are shown:— the value of  $x$ —that is to say, the proportion according to which the chemical decomposition must take place according to the theory of Guldberg; the thermic effect calculated from that decomposition; and the thermic effect found by experiment:—

Reagents.	$x$ .	Thermic effect.	
		Calculated.	Found.
		thermal units.	thermal units.
NaO, SO <sup>3</sup> ..... $\frac{1}{8}$ HO, NO <sup>5</sup>	0·121	— 462	— 452
" ..... $\frac{4}{1}$ "	0·232	— 828	— 808
" ..... $\frac{1}{2}$ "	0·423	— 1331	— 1292
" ..... $\frac{2}{1}$ "	0·667	— 1773	— 1752
" ..... $\frac{1}{1}$ "	0·845	— 1974	— 2026
" ..... $\frac{3}{1}$ "	0·903	— 2019	— 2050
NaO, SO <sup>3</sup> ..... $\frac{2}{1}$ HO, NO <sup>5</sup>	0·845	— 1974	— 2026
NaO, 2 SO <sup>3</sup> .....       "	0·742	— 982	— 978
NaO, 3 SO <sup>3</sup> .....       "	0·667	— 714	— 664
NaO, 4 SO <sup>3</sup> .....       "	0·607	— 551	— 520
$\frac{1}{2}$ NaO, SO <sup>3</sup> + $\frac{1}{2}$ NaO, NO <sup>5</sup> . $\frac{1}{4}$ HO, NO <sup>5</sup>	0·167	— 561	— 546
"       "       "       "       "       "	0·271	— 786	— 761
"       "       "       "       "       "	0·371	— 933	— 968
NaO, NO <sup>5</sup> ..... $\frac{1}{1}$ HO, SO <sup>3</sup>	0·333	+ 298	+ 288
" ..... $\frac{2}{1}$ "	0·458	+ 348	+ 379
NaO, SO <sup>3</sup> ..... $\frac{1}{2}$ HCl ..... " ..... $\frac{1}{1}$ " ..... " ..... $\frac{2}{1}$ " ..... " ..... $\frac{4}{1}$ " .....	0·423 0·667 0·845 0·921	— 1279 — 1691 — 1870 — 1917	— 1247 — 1682 — 1878 — 1896
NaCl ..... $\frac{1}{1}$ HO, SO <sup>3</sup>	0·333	+ 257	+ 244
" ..... $\frac{2}{1}$ "	0·458	+ 292	+ 336

We see that in every case the agreement between calculation and observation is as satisfactory as the degree of accuracy obtained in the measurement of thermic effects will admit of.

The value denoted by  $k$  in the formulæ of Guldberg is no other than the ratio of the avidities of the two acids which act simultaneously. If  $a$  and  $a'$  represent the avidities of the two acids A and A' for the base B, the energy of the decomposition of BA by A' is  $k = \frac{a'}{a}$ , and the energy of the inverse reaction of A on BA' is  $\frac{1}{k} = \frac{a}{a'}$ .

If the two acids have the same avidity  $k=1$ , then

$$x = \frac{\alpha\beta - \gamma\delta}{\alpha + \beta + \gamma + \delta};$$

and if  $\gamma = \delta = 0$ ,

$$x = \frac{\alpha\beta}{\alpha + \beta};$$

we then recur to the formula of Berthollet, which is thus confirmed only in the particular case in which acids of which the avidities are equal are compared.

The author terminates this memoir by the following summary.

1. When two acids act simultaneously in an aqueous solution on a base of which the quantity is insufficient to neutralize them, that base is divided between them in such a manner that they form two salts, and a part of the two acids remains free.

2. Hence it follows that when on a salt an acid reacts of which the heat of neutralization is greater than that of the acid contained in the salt, the reaction is accompanied by a disengagement of heat; in the inverse case there is, on the contrary, an absorption of heat.

3. The division of the base between two acids is not effected according to the law of Berthollet—that is to say, proportionally to the number of equivalents of the two acids.

4. The division does not take place in the ratio of the affinities of the base for the two acids, if we take as a measure of these affinities their heats of neutralization.

5. The force with which the acids tend to be neutralized is what I call *avidity*.

6. When an equivalent of a base is in the presence of two acids in an aqueous solution, the amount of each of these acids in the mixture being one equivalent, the base divides itself between them according to the ratio of the avidities of the two acids.

7. If the acids are in a different proportion, the base divides

itself between them according to their avidity and their number of equivalents; the mode according to which this division is effected is expressed with satisfactory exactitude by M. Guldberg's theoretical formula\*.

LX. *On Thermodynamics.* By the Rev. J. M. HEATH†.

THE variations in the temperature of a body which occur during its expansion and condensation had long been regarded as a problem in physics, to be determined by purely experimental and inductive methods. Until comparatively quite recent times it was held that the cold produced in rapidly expanded air, and the permanent cold in the permanently rarefied air of the upper regions of the atmosphere, were alike caused by the rarefaction itself, and that the capacity of a body for heat increased with its rarity. This doctrine is now abandoned; and it is now held that the energy or quantity of action of a force which causes a condensation is the cause and the measure of the evolution of heat in the body which is observed to follow.

From the moment when this doctrine was regarded as established by a sufficiently probable induction to entitle it to be made the basis of future inquiry, the science, thenceforth to be called the Science of Thermodynamics, had passed from the category of inductive sciences and had virtually become a branch of dynamics, the business of which is the deduction of the consequences of previously admitted first principles in their application to particular cases. The exhibition of heat was now a consequence of the action of force; and it was apprehended that its production would be found to follow the same laws as connect the *vis viva* of a system of particles with the quantity of action which generates it. Now the quantity of *vis viva* which a given energy will generate is perfectly well known. It only remained, therefore, to ascertain whether the empirical law connecting the quantity of heat evolved in a given condensation with the con-

\* [It is probable that, if the author has considered himself able thus to generalize conclusions from observations which only comprise so small a number of cases, it is because these conclusions seemed to him justified by the very great number of other experiments which he has made and intends to publish shortly. Without that they would not be sufficiently proved. As we have already remarked in a preceding note, the ratio according to which soda divides itself between sulphuric acid on the one side, and nitric and hydrochloric acids on the other, might be explained simply by the tendency of the first acid to form a bisulphate, without its being necessary to introduce a difference of energy or avidity. In order, however, to be able to form a judgment on this theory, we must wait for the rest of the memoirs announced by M. Thomsen.—C. M.]

† Communicated by the Author.



densing energy was identical with the deductive dynamical law, and the whole question would be definitively settled.

In two articles previously communicated to this Magazine, I have shown that the assumption that the heat developed in a compression is proportional to the whole force externally applied to the surface of the gas, multiplied by the compression, is erroneous, because the externally applied force necessarily includes a part, called in D'Alembert's Principle "the lost force," which has been already included in the value of the internal forces of the gas, and that it is the remainder only of the applied forces, commonly called the "effective forces," which can produce any such effect as heat or *vis viva* affecting the temperature or total energy of the gas. I shall now complete what I have to say upon this subject, by showing that the same assumption (the proportionality of the heat to the whole impressed force) is equally at variance with the laws of dynamics, which show that the *vis viva* produced is proportional only to the action of the effective forces.

Let  $M$  be the mass of the gas, and let it be contained in a cylinder the area of whose section is unity, in which a piston moves vertically, loaded with a weight  $P$  pounds. Let  $v$  be the depth of gas between the piston and the bottom of the cylinder, and  $Q$  the upward pressure upon the lower surface of the piston, resulting from the expansive force of the gas.

The forces  $P$  and  $Q$  may be considered to be distributively applied to every separate particle in the gas. The force, therefore, which acts upon one such particle ( $m$ ) will be  $\frac{1}{M}P$  acting vertically downwards, and  $-\frac{1}{M}Q$  acting upwards; or the whole

force will be  $\frac{1}{M}(P-Q)$ . Let  $r$  be the distance between two adjacent molecules situated on the same vertical line; then  $\pm dr$  will be the distance by which such particles will approach or recede from each other in consequence of any condensation or expansion which may take place in the vertical direction; but  $v$ , the depth of the gas,  $=\sum r$ , the sum of all the distances of adjacent particles situated in one line;  $\therefore dv = \sum dr$ . Now  $dv$  is the quantity by which the piston descends or ascends during condensation or expansion of the gas; it is equal, therefore, to the sum of all the extensions or contractions of the constant distance  $r$  between all the particles upon the same vertical line.

The equation for determining the *vis viva* generated by any system of forces is  $\frac{1}{2}\sum mv^2 = \int \sum P dp - \int \sum Q dq$ , where  $P$  and  $Q$  are respectively the forces which aid and those which resist any par-

ticular motion actually taken by the particles to which they are applied;  $dp$  and  $dq$  are the motions of the particles. Applying this equation to the foregoing case, we get

$$\frac{1}{2} \Sigma mv^2 = \int \Sigma \frac{1}{M} P dr - \int \Sigma \frac{1}{M} Q dr = \int P dv - \int Q dv = \int (P - Q) dv.$$

It appears, therefore, that, if the identity of the heat evolved by condensation with the *vis viva* generated by the condensing energy is to be maintained, we must take  $(P - Q) dv$ , the effective part of the applied forces, and not  $P dv$ , as the measure of the heat produced. This is in strict conformity with what I have contended for from other considerations.

Milland, May 12, 1870.

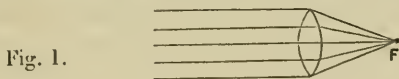
LXI. *On an Optical Illusion.* By J. L. TUPPER\*.

*To Professor Tyndall, F.R.S.*

DEAR SIR,

AS the following fallacious "Solution" that appeared in No. 251 of 'The English Mechanic' has not found correction, and as there are points of real interest connected with a true solution of this not new, but, so far as I know, yet unexplained optical illusion, you will perhaps favour me with your attention to the question.

"[898.] *Optical Illusion.*—I beg to forward the following solution:—It is known that if an object be placed at the focus  $F$  (fig. 1) of a converging glass, the rays will be so refracted by the glass that, after going out of it, they will become parallel; if the object be *further* than the focus (fig. 2), the rays will meet in a point after they have passed the lens; and if *nearer*, they will diverge (fig. 3); hence results that the rays of light proceeding

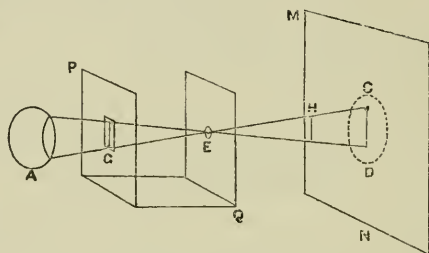


from certain objects, refracted through the crystalline of the eye,

\* Communicated by Professor Tyndall.

must converge to a point, or there would be no distinct picture of them on the retina. An object, to become visible, must then be placed further than the principal focus of the eye; if an object be placed between that focus and the eye, it becomes directly invisible (*p. ex.* the eyelashes); but that object may intercept the rays emitted by another visible object situated further—in a word, it may act as a *screen*. These particulars understood, let A (fig. 4) be the eye, M N a white illuminated surface, P Q the

Fig. 4.



bent card. It is easy to understand that of all the white rays emitted by the surface M N, those alone that are situated on the circle C D can enter into the eye, crossing at the aperture E. If, now, an object be placed on the cone E C D, this object, acting as screen, will intercept some rays of the circle C D, and the eye will perceive in black the image of this object H on the circle. The same must take place when the object is in G, provided it be reversed; then, as all the rays cross at E, the object G will intercept the same rays as H (supposed E G—E H), with that difference, that at G the object, being directly invisible, can only act as a screen, while at H it could at the same time act as a screen for the surface M N and be visible, if it is in the conditions of distance above mentioned. This will explain, then, why the object G is projected in M N in an inverse position.—MICHAEL.”

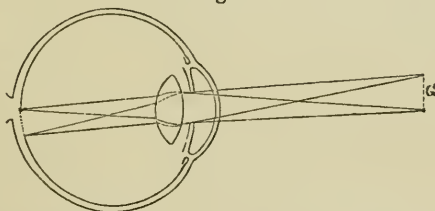
Here the writer explains the erect appearance of the inverted pin G by identifying it with the erect appearance of the erect pin H, as seen by crossing rays—quite forgetting that, if objects ordinarily appear in their true position, a special arrangement which inverts them before they reach the eye will make them appear reversed, that H therefore ought to appear reversed; and that “why it does not” demands explanation as much as “why G does.” He has, by employing two cones of rays, brought the pin on to the lens in an inverted position, and yet expects that we should see it erect, in defiance of hourly experience, that just as objects stand upon the lens so they are seen.

What has to be shown is, how the position of an object can appear the reverse of what it is upon the lens: this he has not attempted. Another singular fallacy is where he postulates G too near for vision (because its rays do not converge upon the retina), and then supposes H (because at a proper seeing-distance) made visible by these self-same rays. If ray A G cannot do the work of vision for G because of non-convergence on the retina, how can it do it for H? His second cone is useless unless we suppose a sensorium at E.

Again, he supposes the light which comes through the hole E reflected from a surface M N beyond the hole, and hence derives his second cone and his means of solution. But it is not necessary to the success of the illusion that light proceed from any such surface, or from any distance beyond the hole; neither is such hole required, any radiant point (such as a spark) being in all respects sufficient. Thus the basis of solution is cut away.

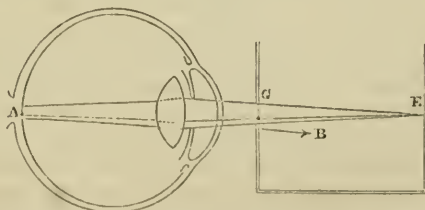
I will now explain the phenomenon by simply following the rays to the retina, first alluding to normal vision, fig. 5, where,

Fig. 5.



the eye seeing the pin G under ordinary conditions, every point in the object delivers its cone of rays (whose base is just within the pupil) to the crystalline, whence, after refraction, the rays pass in another cone whose apex is in the retina (only two cones, depicting the point and head of the pin, are figured). Here the impress of the object on the retina is not only the reverse of the object's real position (or of its position on the lens), but it is moreover the strongest retinal impress that the object can pro-

Fig. 6.



duce, its *every point* being depicted by the convergence of an entire cone of rays. Now (fig. 6) trace the rays as, under the



conditions of the experiment, they pass in a cone from the radiant point E, where they surround the pin, and proceed to the lens, whence, after refraction, they slightly converge and end on the retina in a circle of light containing a dark space corresponding to a section of the pin. But observe here, the impress is not erect, as in fig. 5; neither is every point in its outline depicted by the convergence of a cone of rays (as in fig. 5), but, on the contrary, it is obvious that no more than one ray can go to the depicting of any one point of this impress, which is consequently the faintest that can be produced.

Now, whilst it is manifest that two diametrically opposed positions of the pin on the sensitive retina will produce perceptions of diametrically opposite position, the erect pin on retina fig. 5 giving perception of an inverted pin, and the inverted pin on retina fig. 6 giving perception of an erect pin (and so explaining the illusion), it is equally clear that in fig. 5 there will be the same faint impact of rays on the retina that we find in fig. 6, seeing that there is nothing to hinder a cone of rays from any radiant *point* impinging on retina fig. 5, inasmuch as fig. 6 differs only from fig. 5 in point of deficiency; *i. e.* retina fig. 5 has all the light of retina fig. 6, and a great deal more. Now the experiment is here interesting because, by a special arrangement, it makes this faint impact of light appreciable; and this it does by excluding the more vigorous impact of light; *i. e.* it negatives delineation by cones and admits of it by single rays only; and it shows once more, supposing perception to be *appreciated nervous differentiation*, how such differentiation may exist unappreciated.

For it may, as I shall now show, be proved that both impressions, the stronger and the fainter, do indeed coexist upon the retina, and that the exclusive appreciation of the former is owing solely to its greater intensity. For by introducing the means of delineation by cones of *faint light*, both images (the one erect, the other inverted) may be made visible at once. At E, fig. 6, have a hole ( $\frac{1}{8}$  inch diameter) in the card and cover it with transparent paper. The faint light from this enlarged source will depict all points of the pin by *cones of rays*, which cross a little behind the crystalline, and consequently give a tracing of the pin head upward on the retina, head downward to perception. Now let a small hole be pierced in the transparent paper to do the office of radiant E, *i. e.* to delineate by single rays; and a little nice adjustment, somewhat increasing the distance of G from the eye, will bring this single-ray tracing of the pin head downward on the retina, and head upward to perception. So that we have on the retina at the same time two impressions in contrary positions, and consequently perceive two pins in contrary positions, each retinal impression being respectively reversed

to the mind. To procure this result we first fix the eye upon the radiant point E till we see the pin head upward, as in the illusion; then steadily increasing the distance, as stated, we presently get sight of the actual pin as it is, head downward, with now a little light circle in its head, and a little upright pin within that circle. Remove the transparent paper, and the bright cones obliterate the smaller image; or cut off the cones by opaque paper, still retaining the radiant point, and the larger image vanishes. It is only by reducing the intensity of the cone-depicted image that we make the other image appreciable.

The deductions from these induced facts are, first, concerning erect vision with an inverted image.

However conclusive the reasoning against there being any direct deliverance to the retina, or other neurine, as to the position of the object, however consistent the theory that experience is the sole interpreter of sensory excitement, and that there never is "an inverted retinal image," inasmuch as there is *no image* till experience has made it (made it in accordance with muscular evidence, and so erect), however conclusive all this to reason, by various analogies, direct experimental evidence has been wanting. It is here supplied. For we have here one object before us and two opposite perceptions of its position at the same moment, a present voucher that there is no direct deliverance from the object as to its position. If there were (as still contended by a high authority) "a law" by which intuitively we interpret an effect on the retina in behoof of the object's position, we should here see both images in one and the true position. But we do not. And what is it we do? We fall back upon hitherto experience, and interpret the retinal effect accordingly. It is of no avail that the pin falls on the retina in its true position; we must see it as we have hitherto found it.

A second deduction regards "the law of visible direction," by which, it is said, an object is seen in the direction of a perpendicular to the retina at the object's point of incidence; and it is cited by its promulgator to prove the sense of vision, primary and unacquired; therefore its operation should be absolutely efficient and unindebted to experience, unconditionally indicating the direction of the object. But (fig. 6) the law proves fallacious, the object not being in the direction of the perpendicular AB: it is seen where it is not. That it is seen under novel and unexperienced conditions cannot be urged without admitting experience as a prerequisite to sense of direction.

Thirdly, there are some points of interest in regard to focus, which I omit for want of space, but would observe that the pin neither is, nor is presented as in focus, its outline being everywhere jagged with rays of light. I have shown the attempt to

increase the focal distance by supposed "projection" on the plane M N illusory, and will only add that, while all similar vague theories of "projection" or out-seing are begging the question when the question is of vision, in this case it is obvious that as the plane M N was removed further off, the image thus projected would increase in size; and if the projection represented the appearance as proposed, the pin would appear larger: but it proves otherwise.

It may be noticed that this analysis demonstrates the peculiar effect of what I call single-ray delineation on the retina, which will not only happen when the radiant point is behind the object, showing it as a shadow, but also when an object, itself reflecting rays, is seen through a pinhole. The effect in both cases is faint vision, and an *uninverted* image on the retina, which would result in both cases in *inverted* vision, save that in the latter there has been a previous inversion which sets things right; so that if, while we wonder at the result of this "illusion," we are not surprised at an object seen through a pinhole looking upright, it is not from knowing more of what takes place.

Your obliged Servant,

Rugby School,  
March 18, 1869.

J. L. TUPPER.

*Note.*—A spark at the end of a smouldering thread I have found to exhibit the phenomenon most perfectly. Multiply these sparks, and you multiply the images of the pin; and the same if you multiply the pinholes in the card E, their local relations appearing the same as in nature.

LXII. *On an Electromagnetic Experiment.* By the Hon. J. W. STRUTT, *Fellow of Trinity College, Cambridge*.\*

THE experiment referred to is one described in the *Philosophical Magazine* for July 1869, p. 9, where it was shown that, within certain limits, the magnetizing effect of a break-induced current on steel needles is greater the smaller the number of turns of which the secondary circuit consists, the opposite, of course, being true of the effect on a galvanometer. The ground of the distinction is that the galvanometer takes account of the induced transient current as a whole; while the magnetizing-power depends mainly on the magnitude of the current at the first moment of its formation, without regard to the time which it takes to subside.

But even with this explanation, few, I imagine, would be prepared for the result who had not been accustomed to look at

\* Communicated by the Author.

electrical phenomena in the light of some dynamical theory. It was for this reason that I considered the matter worthy of experimental investigation, the fruits of which were given in the paper referred to. One point, however, still required a little clearing up; and it is this which I now propose to deal with. I mean the mode of action of the condenser, which was employed, as in the inductorium, for the purpose of rendering the break more sudden, and which I had found necessary for the success of the experiment as then arranged. At this necessity I was not surprised; for, according to the indications of theory, the effect was only to be expected when the fall of the primary current is sudden compared to that of the secondary. Now the duration of free transient currents in a circuit varies, *ceteris paribus*, as the self-induction; so that when the number of turns in the secondary is too much reduced, there is danger of the condition not being fulfilled. If it be objected that as much would be gained by improved conductivity as lost by diminished self-induction, I answer this is not the fact, the resistance varying as the number of turns simply, while the self-induction varies as the square of the same number. Besides, I had reasons for keeping the resistance in all cases invariable.

Wishing, however, to obtain the effect without the aid of a condenser, I prepared a quadruple coil by bending into the form of a compact ring a bundle consisting of four No. 16 copper wires, each 70 feet long. Into one of these the current from a Daniell cell was permitted to flow, subject to interruption at a mercury-cup. The secondary circuit consisted of the other three wires arranged consecutively, and of the magnetizing spiral, which contained the needle destined to measure the effect of the momentary current. The three wires could be joined so that the current circulated the same way round them all (*a*), or so that in one of them the direction was the opposite to that in the two others (*b*). It will be seen that the resistance was always the same, the only change being in the coefficient of mutual induction (*M*), and of self-induction of induced circuit (*N*). In the former paper it was shown that the *initial* induced current, being proportional to  $\frac{M}{N}$ , is three times as great in (*b*) as in (*a*) (a sufficient suddenness of break being assumed), while the total currents are in the reciprocal proportion. In carrying out the experiments, I submitted the needle (a *new* one in each measurement) to the action of six break-induced currents, always removing it from the spiral when the battery contact was made. In this way a more constant result is obtained than from one discharge only, which is liable to vary from slight differences in the character of the break. The needle was then swung by a silk fibre and the number of



complete vibrations in a minute observed. The numbers were :—

Arrangement (a)	.	$4\frac{1}{2}$ , 6, 6	: mean	5.
„ (b)	.	21, 19, 19	: „	20.

The superior efficiency of (b) is very conspicuous.

There is another way in which the subject may be investigated. If the secondary current containing a galvanometer be broken so quickly after the primary that the induced current has not time during the interval sensibly to diminish, the deflection of the needle may be considered to measure the initial value of the induced current. To carry out this experiment properly would require rather elaborate apparatus, on account of the necessity of a *constant* interval of time between the breaks. The contrivance that I used was of home manufacture and very rough, and acted by the almost simultaneous withdrawal of wires from two mercury-cups. The secondary circuit in case *a* consisted of the two wires of a large coil, A, joined consecutively, and of a short wire galvanometer. Iron wires were inserted in A in order to increase the duration of the induced current. In *b* only one wire of A was used, the resistance being made up by the substitution of another wire, whose self-induction might relatively be neglected. The total currents in the two cases would be as 2 : 1, and the initial currents as 1 : 2. The deflections of the galvanometer-needle were rather irregular; but the sum of ten throws in case *a* was  $317^\circ$ , while in *b* it rose to  $480^\circ$ , so that there could be no doubt as to the reality of the phenomenon.

Returning to the experiments with steel needles, I thought it desirable to compare the permanent magnetisms developed in two cases where the initial currents were equal. With this object the primary current (originating in a Grove cell) was passed through two wires,  $Q_1$ ,  $Q_2$ , of the quadruple coil combined for self-induction. The induced circuit included  $Q_3$ ,  $Q_4$  and the magnetizing spiral. The arrangements in other respects being as before, I obtained—

13, 14, 15, 16, mean  $14\frac{1}{2}$  vibrations per minute\*.

$Q_2$  was now removed from the primary circuit (the resistance being made up to former value) and  $Q_4$  from the induced circuit. The numbers now were—

11, 10, 12, 12: mean  $11\frac{1}{2}$ .

$Q_2$  being next replaced, but not  $Q_4$ , there resulted,—

35, 35, 33: mean 34.

\* The experiments were not made exactly in the order here adopted for convenience, but were broken up.

In the first two arrangements the initial currents would be equal, while in the third they would be doubled.

On the whole, I think these experiments confirm the view that the acquired magnetism depends principally on the initial current. The exact laws regulating the connexion between the current and the magnetism produced by it are doubtless complicated, and not of much interest. The facts here detailed should, however, be borne in mind by any one who wishes to pursue this subject, and they do something towards explaining the discordant results of previous experimenters.

And now as to the effect of a condenser. Considering that, in consequence of the length of wire in the quadruple coil, the duration of a current in it, even under arrangement *b*, must be much greater than the time occupied by the break, or, which is the same thing, the duration of the spark, I did not anticipate that a condenser (whose plates were connected with the breaking-points) would have any influence. But to my surprise I found that although, of course, the superiority of *b* was not disturbed, the magnetic effects were all increased. The explanation is, I believe, to be found by an examination of what takes place in the two circuits after the primary current is thrown on the condenser by the removal of the wire from the mercury-cup.

The charge (or discharge) of a condenser through a circuit endowed with sensible self-induction was first investigated by Sir W. Thomson (see *Phil. Mag.* vol. v. p. 393, or *Wiedemann's Galvanismus*, vol. ii. p. 1007). There are two cases according to the comparative magnitudes of the three elements of the problem, which are (1) the capacity of the condenser, (2) the self-induction of the circuit, (3) the resistance of the same. If these, reckoned in absolute measure, be denoted by *S*, *L*, *R* respectively, the motion of electricity is of an oscillatory character if

$$S < \frac{4L}{R^2};$$

otherwise the charge is completed and equilibrium established without a retrograde motion. The motion of a pendulum in a viscous liquid is exactly analogous and may serve as an illustration. If the viscosity of the liquid exceed a certain limit, the pendulum withdrawn from the vertical and then let go will subside gradually back again into its position of equilibrium without ever passing it; but if the viscosity be small, rest is only attained after a number (theoretically indefinite) of oscillations of continually decreasing amplitude. To our case of currents mutually influencing one another, Thomson's calculations are not immediately applicable; indeed the exact solution would be

rather complicated\*. However, we are concerned principally with the first part of the electrical motion, the manner in which the currents wear down under the action of resistance being of subordinate importance. Now it appears that, if the motion be decidedly of the oscillatory type, the first few oscillations will take place almost uninfluenced by resistance; and on this supposition the calculation becomes remarkably simple.

$L, M, N$  being the induction-coefficients, as before, let the total flow of electricity in the two circuits from the moment of the break be  $x, y$ , so that the currents at any moment are  $\frac{dx}{dt}, \frac{dy}{dt}$ .

Then the equations to the currents are

$$L \frac{d^2x}{dt^2} + M \frac{d^2y}{dt^2} + \frac{x}{S} = 0, \quad . \quad . \quad . \quad . \quad (1)$$

$$M \frac{d^2x}{dt^2} + N \frac{d^2y}{dt^2} = 0, \quad . \quad . \quad . \quad . \quad (2)$$

where  $S$  is the capacity of the condenser.

Eliminating  $y$ , we get

$$\left(L - \frac{M^2}{N}\right) \frac{d^2x}{dt^2} + \frac{x}{S} = 0.$$

The oscillation in the primary wire is accordingly the same as if the secondary were open and the self-induction changed from  $L$  to  $L - \frac{M^2}{N}$ . (2) gives immediately the connexion between  $x$  and  $y$ ,

$$M \frac{dx}{dt} + N \frac{dy}{dt} = \text{const.},$$

which shows that the currents in the two circuits oscillate synchronously, the maximum of one coinciding in time with the minimum of the other. Since  $\frac{dy}{dt} = 0$  at the moment of break,

the constant of integration must be equal to  $M \frac{dx}{dt_0}$ ;  $\frac{dx}{dt_0}$  denoting the value of the primary current at or before the break. Accordingly

$$\frac{dy}{dt} = \frac{M}{N} \left( \frac{dx}{dt_0} - \frac{dx}{dt} \right);$$

so that when after half an oscillation  $\frac{dx}{dt} = -\frac{dx}{dt_0}$ ,

$$\frac{dy}{dt} = 2 \frac{M}{N} \frac{dx}{dt_0}.$$

\* It would depend upon a cubic equation.

This is its maximum value, and is *double* of that which would be generated by a simple stoppage of the primary current, however sudden. In this way I am inclined to explain the increase of effect produced by the condenser. It is true that, having reached its maximum value, the secondary current rapidly declines and then changes sign; but from what is known of the behaviour of permanent magnets when submitted afresh to the action of magnetizing force, it does not seem likely that much disturbance would arise from this cause.

A more plausible objection may be founded on the exceeding rapidity of the oscillations, for *some* time must be necessary for the magnetization of steel. Indeed in our case the period of oscillation is unusually short, on account of the smallness of  $L - \frac{M^2}{N}$ . When the two circuits are composed mainly of wires coiled side by side,  $L, M, N$  are approximately equal, and therefore  $L - \frac{M^2}{N}$  very small compared with either of them. The current is then transferred, with almost indefinite rapidity, from one wire to the other and back again.

I made some experiments to examine this point, which will also serve as examples of the general increase of magnetic effect produced by a condenser. The primary current from a Grove's cell was passed through the two wires of coil A joined consecutively, and then through one wire of a similar coil B. According as the wires of A are joined,  $L=5$  or 1. The induced circuit included the other wire of B and the magnetizing spiral, so that  $N=1$  and  $M=1$  approximately. In the first case, therefore,

$$L - \frac{M^2}{N} = 4,$$

while in the second it is very much smaller. The experiments, conducted in other respects as before, gave the following results:—

$L=5$ , without condenser	. 16, 13, 13, 14; mean 14.
$L=5$ , with condenser	. . 24½, 28, 31, 28; „ 28.
$L=1$ , without condenser	. 14, 15, 13½, 14; „ 14.
$L=1$ , with condenser	. . 17, 18, 19, 21; „ 19.

It will be seen that, while without the condenser it made little difference whether  $L=1$  or  $L=5$ , the increase produced by the condenser is much greater in the latter case. This is so far in agreement with the explanation just given; but I confess I should have been better satisfied had the influence of the condenser been less marked when  $L=1$ . In order that the reader may better



judge of the correctness of the view here taken, I subjoin an estimate of the period of oscillation in the actual arrangement of the experiment.

The time of a complete oscillation of a current in simple connexion with a condenser  $= 2\pi \sqrt{LS}^*$ , where  $L$  and  $S$  are measured in absolute measure. If  $\tau$  be the time-constant of the circuit,  $L = R\tau$ ,  $R$  being the resistance in absolute measure. Now the capacity called a Farad is  $10^{-13}$  absolute measure, and the B.A. unit is  $10^7$  on the same system.

If therefore we take as practical units the B.A. unit of resistance, and the Farad as unit of capacity,

$$t = \frac{\pi}{500} \sqrt{RS\tau}.$$

$t$  and  $\tau$  are here measured in seconds. The condenser employed (made by Elliott Brothers) had a capacity of half a Farad, so that  $S = \frac{1}{2}$ . For one wire of coil A or B,

$$R = \frac{1}{4},$$

$$\tau = .004\dagger.$$

For one of these wires in simple connexion with the condenser the time of oscillation would be

$$\frac{\pi}{500} \sqrt{\frac{1}{4} \times \frac{1}{2} \times \frac{4}{1000}} = \text{about } .00014''.$$

Comparing this with the value of  $\tau$ , we see that the first ten or so oscillations would be comparatively unaffected by resistance. By what has been proved, the time, when  $L = 5$  (in different unit of course), must be just double of this, or  $.00028''$ .

The action of the condenser in the inductorium is very imperfectly explained in the text-books, and is no doubt in many cases rather complicated. From the reasoning of this paper, it appears that it is by no means a complete account of the matter to say that the advantage derived from the use of the condenser depends only on the increased suddenness with which the primary current is stopped. In a complete investigation (which I do not mean to enter on here) a distinction would probably have to be made, according as the secondary circuit when open allows the passage of a spark or not, or, as a third case, is completely closed. I would, however, remark that a good deal of misapprehension arises in this and similar cases from forgetting that a condenser is powerless to make away with electrical energy. Such energy may be disposed of in the form of a spark, or it may be converted

\* The effect of resistance being neglected.

† The value of  $\tau$  previously given for coil A is erroneous.



itself. Now, however, I admit that it is absolutely necessary that the equation ( $\alpha$ ) should be satisfied by the solution of every problem which relates to motion of an incompressible fluid for which  $udx + vdy + wdz$  is an exact differential. I therefore fall back upon the first of the two solutions in the April Number, which appears, for reasons I am about to adduce, to have been too hastily rejected.

The infinite wave might be accounted for in the same manner as the continually increasing velocity of a pendulum, or an elastic spring, which is subject to external action having the same period as its own. Any disturbance of the waters of the supposed ocean would generate waves which of themselves would be propagated with a velocity depending on the uniform depth; but the period of the tide-wave, or the rate of its propagation, is determined by the mean relative periodic motion of the moon about the earth. If, however, the depth of the ocean should be such that the rate of propagation depending upon it is equal to the rate of the moon's relative rotation, the waters might receive continual accessions of velocity, just as in the case of the pendulum or spring above mentioned. It may be presumed that the infinite velocity given by the solution in question for a certain value of the ocean's depth is generated under these conditions.

This view is confirmed by actually calculating for the case of an equatorial canal the depth  $H$ , which corresponds to a rate of propagation equal to that of the moon's rotation about the earth on the supposition that the rate  $= \sqrt{gH}$ . I find by this calculation that  $H$  is equal to 12.6 miles. This agrees closely with the depth (12 miles) of an equatorial canal in which, according to Mr. Airy's mathematical investigations, an unlimited tidal elevation would be produced. (See *Phil. Mag.* for April, p. 267.)

Since we found that in an unbounded ocean the infinite wave is generated if the depth be 8.5 miles, the rate of propagation of the lunar tide along the equator would appear to be greater in such an ocean than in an equatorial canal in the ratio of  $\sqrt{12.6}$  to  $\sqrt{8.5}$ ; that is, of 1.22 to 1.00 nearly. This increment may be due to the undulations which, according to the solution now under consideration, take place in directions *transverse* to the equator, in the same manner as, in the theory I have proposed of the velocity of sound, the rate of propagation is augmented by transverse vibrations, and very nearly in the same ratio. (See an Article in the *Phil. Mag.* for June 1866, and 'Principles of Mathematics and Physics,' pp. 214-224.)

I have obtained the same solution by means of particular integrals of the three differential equations of which  $u$ ,  $rv \cos \lambda$ , and  $rw$  are the principal variables, the other variables being  $r$ ,  $\theta$ , and  $\lambda$ . In this method the relations between the arbitrary con-

stants contained in the three integrals are determined so as to satisfy the conditions of integrability of

$$u dr + r v \cos \lambda d\theta + r v d\lambda.$$

Although the foregoing solution may be far from being applicable to the case of *actual* tides, still the problem is one which it is necessary to be able to solve strictly before a mathematical theory of such tides can be entered upon.

Cambridge, May 23, 1870.

LXIV. *On the Connexion between the Electrical Insulating-power and Chemical Composition of various kinds of Glass.* By F. L. EKMAN, Junior Professor at the Royal Technological Institute in Stockholm\*.

IT has long been known that the electrical insulating-power varies greatly in different kinds of glass; but the causes of these differences have been but little investigated. The question, however, is of considerable practical importance, and I therefore, at the suggestion of Professor E. Edlund, undertook to inquire whether the difference in the insulating-power of various kinds of glass might not be ascribed to differences in their chemical composition. In this examination I employed five specimens of glass, supplied to me by M. Oeller, electrical-instrument maker, two of which have been found by him unfit, but the remaining three good for electrical purposes.

Most chemists, I imagine, in order to answer such a question, would, like myself, in the first place direct their attention to those chemical relations in the glass which can produce differences in the qualities of the surface. A species of glass which is, within reasonable limits, rich in silica, potash, or lime ought to prove a better insulator than a lime glass containing much soda, simply because it is harder, and therefore preserves a perfectly smooth surface better than the latter. But the insulating-power of glass may depend in a still higher degree on its power to resist the effect of chemical agents; for humidity and carbonic acid in the atmosphere are at all times present to corrode it. The new substances that are thus formed on the surface of the glass may be themselves inferior to glass as insulators, and moreover may contribute to the production of a conducting stratum of damp upon its surface. It is well known that glass in the form of powder is rapidly affected by water; the examination of glass which has lain long in the earth or in water has proved

\* Report made to the Session of December 1869 of the Royal Academy of Sciences in Stockholm. Communicated by the Author.



beyond doubt that the same change takes place, though far more slowly, on a smooth surface of glass. It is known also that there are kinds of glass which in moist air speedily become covered with a visible coating that dims both the brilliancy and transparency of the glass. That such kinds of glass, which must be considered absolutely faulty in their composition, must be unfit not only for optical but for electrical purposes is self-evident; but it appeared to me probable that similar changes in the surface of glass even of unexceptionable composition might take place in the air, sufficiently minute, indeed, to escape observation, but sufficiently serious to affect the electrical insulating-power, and that the differences between various specimens of glass as insulators may accordingly depend chiefly on their different chemical durability.

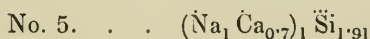
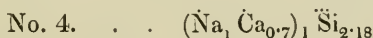
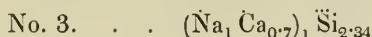
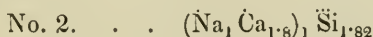
The expression for the chemical durability of glass is to be found in its equivalent formula. The composition of ordinary glass varies on each side of the mean formula ( $\text{MO} + 2\text{SiO}^3$ ), where MO signifies alkali and lime. The kinds of glass in which the amount of silica exceeds that expressed by the formula possess usually a greater degree of chemical durability, and *vice versa*; but that durability depends also in a great measure on the mutual relation between the alkali and lime, so that, at least to a certain limit, it is increased the more lime is substituted for alkali.

On subjecting the above-mentioned kinds of glass to analysis, I found their composition to be as follows:—

	Good for electrical purposes.			Unserviceable for electrical purposes.	
	Glass No. 1.	Glass No. 2.	Glass No. 3.	Glass No. 4.	Glass No. 5.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Silica . . .	75·7	71·2	76·1	74·2	72·8
Sulphuric acid.	0·6	1·0	0·7	0·8	1·0
Arsenic acid . .	..	..	0·2	0·3	
Oxide of lead . .	..	..	..	0·3	
Oxide of iron.					
Proto - sesqui-oxide of man- ganese . . }	0·4	1·5	0·4	1·0	0·9
Alumina . . }					
Lime . . .	8·9	15·2	8·3	8·7	9·3
Magnesia . .	0·1	0·4	0·1	0·1	0·3
Soda . . .	2·9	9·7	11·8	13·8	15·6
Potash . . .	11·2	1·0	2·9	0·4	1·0
	99·8	100·0	100·5	99·6	100·9*

\* The amount of lime found is probably somewhat too great.

In calculating the equivalent formulæ, I have deducted from the alkalies as much as is necessary to form a neutral salt with the sulphuric acid found. The remaining alkali I have reckoned in equivalents of that alkali which was present to the greatest amount in the glass; and in like manner I have reckoned magnesia and oxide of lead as lime. The small quantities of arsenic acid, oxide of iron, oxide of manganese, and alumina I have neglected. Thus calculated, the equivalent formulæ of the respective kinds of glass will be as follows:—



According to these formulæ, Nos. 1, 2, 3, which were found to be good insulators, must be allowed to possess more chemical durability than the inferior sorts Nos. 4 and 5. The glass No. 3, with the same proportion between lime and alkali as in the last named, contains in fact considerably more silica; No. 1 contains both more silica and more lime; and as for No. 2, it is certainly less saturated with silica, but it contains instead a far greater quantity of lime in proportion to the alkalies than the other kinds of glass. Here I would call attention to the observation of Pelouze, who found that glass of the formula  $(\text{Na}_1 \text{Ca}_{0.4})_1 \text{Si}_{2.3}$  was fretted by the action of water three times as rapidly as glass of the formula  $(\text{Na}_1 \text{Ca}_{1.4})_1 \text{Si}_{1.7}$ .

In order to test experimentally the chemical durability of the specimens of glass before us, I first applied Weber's test\*. I placed pieces of the glass, well cleaned, for a period of twenty-four hours over fuming hydrochloric acid in a closed vessel, and then for some time over lime. When the specimens were taken out, it was evident that the glass No. 5 was the most affected; it was only necessary to hold it up to the light to see that it had been etched. By scraping the glass surface with a blunt knife it appeared that even specimens Nos. 1, 2, and 3 were everywhere somewhat affected, but visibly less than No. 4, and far less than No. 5.

In order more directly to test the durability of the five kinds of glass under the action of the atmosphere, I first exposed weighed specimens, in the form of an impalpable powder, to the

\* R. Weber, "Ueber das Beschlagen und Blindwerden des Glases," *Dingl. Polyt. Journal*, vol. clxxi. p. 129.

action of air free from carbonic acid, but saturated with moisture at the ordinary temperature, and determined the increase of weight after a certain time. The vessel in which the specimens were exposed to the action of moisture was of silvered metal with the surface polished. The same quantity (1 gramme) of each specimen was weighed out; for in this as in all other respects the greatest possible uniformity was observed in these experiments. The following Table shows how many milligrammes of water each specimen absorbed in the time stated:—

		2 hrs.	4 hrs.	6 hrs.	12 hrs.	18 hrs.	24 hrs.
Specimen	No. 1.	4.9	7.7	9.8	12.1	..	17.4
„	No. 2.	3.6	4.8	6.5	..	..	15.2
„	No. 4.	6.9	12.2	..	..	31.4	33.5
„	No. 5.	8.0	..	..	23.3	33.5	

With No. 3 the observation failed; I can say, however, with certainty that it absorbed less water than either No. 4 or 5.

We see, then, that the kinds of glass which were unfit for electrical apparatus were much more hygroscopic than the others, and that the glass which contained much lime was least hygroscopic, whereas that which was relatively deficient in lime and silica was most so. When, after being dried in air (as free as possible from carbonic acid) at  $160^{\circ}\text{C}$ ., the specimens thus tested were reweighed, it was found that Nos. 1 and 2 had almost exactly resumed their original weight, No. 4, on the contrary, weighed 0.0014, and No. 5 0.0022 grm. more than before; and both these specimens appeared in a striking degree agglomerated, evidently in consequence of the powder being glued together by the hydrosilicated alkali formed.

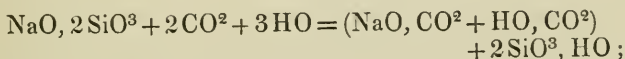
I now placed the specimens over water in a closed vessel containing, instead of air, carbonic acid gas. After a certain time they were placed over lime and chloride of calcium till constancy of weight had been attained, after which they were dried at  $160^{\circ}\text{Cels.}$  and again weighed. The affinity of the specimens for carbonic acid in the presence of humidity is shown in the following Table:—

Glass	No.	Time of exposure to action of car- bonic acid. hrs.	Increase of weight, in milli- grammes, after drying.	
			At ordinary temp.	At $160^{\circ}\text{Cels.}$
	No. 1.	6	7.7	2.7
„	No. 2.	12	..	1.4
„	No. 3.	6	13.6	4.5
„	No. 4.	6	19.4	6.7
„	No. 5.	4	15.8	5.0

Although the time of exposure was not the same for all the

specimens, yet it is evident that the better-insulating glasses were least affected, and that, generally speaking, the glasses display sensibility to the action of carbonic acid in the same order in which they evince readiness to combine with water.

The increase of weight obtained after drying at ordinary temperatures is, in all the specimens, very nearly three times as great as after drying at 160°. Knowing, then, that it is the silicated alkali in the glass that is first attacked by the water, we can explain the phenomena observed by the following formula for the decomposition of glass in presence of a large amount of moist carbonic acid:—



of which last products  $\text{CO}^2 + 3\text{HO}$ , or about two-thirds of the weight of the matter absorbed, disappear at the higher temperature.

Pelouze\* has shown that water extracts from glass powder a soluble alkaline hydrosilicate. I convinced myself that even in humid air it is not only hygroscopic in the ordinary meaning of the word, but that it binds a part of the water chemically. The glass No. 5, reduced to an extremely fine powder, after having stood eight days over lime-water, and then thirteen days over lime and chloride of calcium, still showed 6 per cent., after drying at 100° C. 2·7 per cent., and after drying at 160° 1·5 per cent. increase of the original weight†. Alkaline silicate, as is known, retains water at a temperature of 150°. By testing it for carbonic acid I found only 0·4 per cent. Dry carbonic acid had, as one might have expected, no effect at all upon the glass powder; the carbonic acid can but be absorbed in proportion as it decomposes the hydrosilicates formed by the action of the aqueous vapour.

It was now a matter of interest to subject the glass specimens, which I had received ticketed simply as either “good” or “un-serviceable,” to a more accurate test of their insulating-power, and see whether in that respect they followed strictly the order which their chemical qualities gave reason to expect. This investigation I executed in the physical cabinet belonging to the Royal Academy of Sciences, to the Superintendent of which (Professor Edlund) I am obliged for much valuable advice received on the occasion.

For the purpose, it was necessary to arrange so that the experiments might be independent of the form and size of the

\* *Comptes Rendus*, vol. xliii. p. 117; vol. lx. p. 985.

† When the powder was placed over caustic ammonia I obtained almost identically the same increase of weight as over water.



glasses employed. After the glasses had been well cleaned, two narrow strips of tinfoil coating were fixed parallel to each other on one side of each glass. The one strip which was intended to be charged with free electricity was fastened as far as possible from the edges of the glass, the other was placed in communication with the earth. These tinfoil slips were all of the same size, namely 43 millims. long and 8 millims. broad, with the corners rounded off; the distance between them was 48 millims. This coating having been fixed, the glasses were warmed and covered with shellac varnish, leaving, however, the tinfoil slips and the glass surface between them bare.

The glasses being thus prepared and the lower coating of each connected with the earth, the upper coating was charged from a Leyden jar, to which the same quantity of positive electricity was always communicated by means of a Lane's electrometer. The insulating quality of the glass was now measured by the length of time during which the electricity remained in the upper coating. As the last traces of electricity disappear very slowly from well-insulating kinds of glass, I considered it sufficient to observe how long the charge in the upper coating continued sufficiently strong fully to deflect the gold-leaf in a Bohnenberger's electroscope on the transfer of the first charge from the coating to the electroscope. This test, though it cannot make pretensions to extreme accuracy, is yet abundantly sufficient to establish the order of precedence in which the different kinds of glass must be classed as insulators. The relative moisture of the air varied during the experiments from 47 to 52 per cent., at a temperature of from  $15^{\circ}$  to  $18^{\circ}$  C., and may therefore for these experiments be regarded as sufficiently constant.

The times of insulation thus determined for the different glasses were found on an average to be—

Glass No. 1.	. . .	At least 6 hours.
„ No. 2.	. . .	About 51 minutes.
„ No. 3.	. . .	„ $8\frac{1}{2}$ „
„ No. 4.	. . .	„ 50 seconds.
„ No. 5.	. . .	„ 12 „

The differences were, as we see, strikingly great, and tend on the whole in the direction which one, on chemical grounds, would be led to expect. But here there is a remarkable exception. Specimen No. 2 had proved less hygroscopic and less susceptible to the action of carbonic acid than No. 1, and yet is far surpassed by the latter in insulating-power.

It now appeared to me worth while to try whether any of the badly insulating kinds of glass could be improved by the removal of the old surface of the glass. I tried this experiment

with No. 5. Partly by friction with alcohol mixed with a little hydrofluoric acid, partly by means of crocus-powder, I endeavoured to remove the old surface, and as soon as possible afterwards again tested the insulating-power of the glass. This indeed showed some improvement, but so little that the glass must still be considered utterly unserviceable for the purpose of electrical insulation.

I was accordingly driven to ask myself the question, whether the rapid disappearance of electricity from certain kinds of glass might not, in a far greater degree than is usually supposed, depend on a passage of it through the substance of the glass itself. This led to the following experiments.

The specimens were subjected to the same test of their non-conducting-power that has been above described, with the difference that now also the surface between the tinfoil coatings, previously left bare, was covered with shellac varnish. The relative moisture of the air exhibited no material change. I obtained the following times of insulation :—

Specimen No. 1.	. . .	At least 12 hours.
„ No. 2.	. . .	„ 1½ hour.
„ No. 3.	. . .	About 18 minutes.
„ No. 4.	. . .	„ 60 seconds.
„ No. 5.	. . .	„ 40 „

It appears, then, that the insulating-power of the glasses had, indeed, been increased by covering their surfaces entirely with shellac ; but that increase appeared to bear a certain proportion to the insulating-power displayed by the glasses while the surface between the tin-foil coatings was still left bare. The increase is therefore very inconsiderable for the inferior kinds of glass ; and, indeed, the different sorts appear to differ in insulating-power as widely as before.

These experiments appear to me to justify the conclusion *that in the specimens of glass tested the electricity took its way principally through the substance of the glass itself, and that the nature of the surface was of relatively subordinate importance.*

To verify more fully the correctness of this conclusion, I made the following experiments with specimens 2 and 5, which were plates of nearly the same thickness and circumference. In the middle of the plate a circle of tinfoil, 7 centims. in diameter, was fastened, and a similar one on the opposite side of the plate, after which the whole remaining surface of the glass was varnished. The distance then between the tinfoil coatings was the thickness of the plate ( $3\frac{1}{2}$  millims.), reckoned through the substance of the glass, but at least 12 centims. along its surface. I now insulated the plate 5, put one (*i. e.* the upper) coating

in communication with the inner coating of a Leyden jar charged as in the preceding experiments, and touched the lower coating with a conductor which communicated with the outer coating of the Leyden jar. I could then receive sparks from the coating of the plate at intervals of less than a second, until the Leyden jar was nearly discharged. When the communication between the plate and the jar was kept up constantly, the latter had entirely discharged itself in twenty minutes. The result with the plate No. 2, under exactly the same circumstances, was widely different. When the electricity that first arose in the lower coating by induction had been got rid of, I had to wait several minutes before a scarcely perceptible spark could be obtained, and with continuous communication between the two coated surfaces of the plate and those of the Leyden jar, the latter, after an interval of an hour, was still found to be strongly charged.

It has been remarked that glass, when newly blown, has a conducting outer crust, and only loses its conducting-property by wear and tear after being used for some time. In order to assure myself that the strong conducting-property of the plate No. 5 did not arise from some such cause, I repeated the above-mentioned experiment, only with the difference that the outer coating of the jar communicated with a narrow rim of tinfoil round the edge of the whole plate. It is clear that if the conducting-power of the plate depended on its surface and took place along that and under the varnish, the discharge of the Leyden flask ought now to take place about twice as rapidly as before; if, on the contrary, it depended principally on the conducting-power of the glass itself, the discharge ought to take place far more slowly than in the former experiment. The latter was found to be the case; for the Leyden jar was still strongly charged after an interval of three quarters of an hour.

If we now go back to the analyses in order to obtain from them some indication of what caused the difference of conducting-power in the substance itself of the different glasses, it is evident that the phenomenon cannot be satisfactorily explained either by the amount of silica or of lime contained, whether we take account of their percentage or of their equivalent proportions. But there is an ingredient the quantity of which in the glass continually increases in the measure in which its electrical insulating-power decreases: that ingredient is soda; and I consider that I may combine the results of this research in the following statements:—

(1) The non-conducting property of the glass does indeed in some measure depend upon the more or less non-conducting

character of its surface, and indirectly on its chemical durability ; but

(2) It depends also on the more or less non-conducting character of its internal mass, and in that respect is inversely proportional to the amount of soda in the glass.

(3) In the case of kinds of glass whose surfaces are not of a peculiarly unfavourable nature, the latter circumstance has a far greater influence than the former on the serviceableness of the glass as an electrical insulator.

It is certainly an unexpected result, that of two oxides so similar to each other in their properties as soda and potash, the one should, in combination with silica and lime, produce an electrically conducting glass, the other an almost non-conducting\*. But as long as we have no certain knowledge in what proportions the silica in the glass combines with the lime and alkali respectively, and of the conducting-power of the combinations thus arising, it appears to me that, from the analyses and experiments here described, we cannot avoid coming to the conclusion just enounced. I do not, however, mean to deny that the more complete saturation of the bases with silica may contribute to the production of the non-conducting character of the substance.

It would be very interesting, in the first place, to have specimens prepared of pure potash-lime-glass and soda-lime-glass of different, but corresponding, equivalent formulæ, and then to subject them to an accurate test of their insulating-power and their other physical properties. As, however, my occupations will probably not allow me time to continue this research, I do not wish to reserve to myself the completion of the work.

Stockholm, May 19, 1870.

LXV. *On the Change in the Radiation of Heat by Roughness of the Surface.* By the late Prof. G. MAGNUS†.

LESLIE‡, who was the first to observe that a body with a rough surface radiates more heat than one with a smooth one, also expressed the opinion that this was conditional on the density of the surface. Yet he urges against this view the circumstance that the limit between hard and soft bodies cannot be fixed.

Melloni§ subsequently also maintained that the altered radia-

\* That potassium as a metal is inferior in conducting-power to sodium has been proved by Lamé, *Pogg. Ann.* vol. c. p. 167.

† Translated from the *Berliner Monatsbericht*, October 1869.

‡ An Inquiry into the Nature of Heat.

§ *Comptes Rendus*, vol. vii. p. 238. *Poggendorff's Annalen*, vol. xlv. p. 57.



tion depended merely on a change in the density of the superficial layer. In support of this view he adduces the fact that in certain substances, such as glass, marble, and agate, he observed no change in the radiation whether the surface were rough or polished, and only in the case of metals was there an increase when they were roughened. As this was observed not only in the case of easily oxidizable metals, but in those of gold and platinum, so that the notion that the change might have been due to a layer of oxide was excluded, Melloni ascribes the phenomenon to metals being compressible, but agate, ivory, or marble not. Experiment teaches, he says, that metal plates which are made by hammering or rolling have a greater density on the surface than in the interior. — Looking at a list of the radiating-powers of bodies, it is at once seen that in general this property is inversely as their respective densities. Hence, if it be assumed that the same law also holds for different densities of one and the same substance, the greater radiation is explained; for, by scratching, the surface becomes less dense, or the interior softer places are laid bare.

In support of this view Melloni adduces the following experiment. He had four plates of very pure silver made—two strongly hammered, and two cast and allowed to cool very slowly in their moulds of sand. These he used as the sides of a quadrangular box with a metal bottom; and in order not to alter the density and hardness of the plates, he soldered them with easily fusible solder. Before being joined, they were polished with pumice-stone and charcoal without using hammer or burnisher. One of the cast and one of the hammered plates were then strongly rubbed in one direction with coarse emery-paper. The sides which had retained their lustre reflected sharp images, the rubbed sides dull and streaked ones. The silver vessel thus prepared was filled with hot water. The deflections which the four sides produced were:—

The hammered and polished side . . .	10°
The hammered and scratched side . . .	18
The cast and polished side . . . . .	13·7
The cast and scratched side . . . . .	11·3

Scratching the hammered plate had thus produced an increase of the radiation, while in the cast plate it had produced a diminution. This unexpected fact appeared to him to prove the correctness of his principle.

We are as much, and perhaps more justified in saying that the surface becomes denser by scratching, than in saying, with Melloni, that it becomes less dense; for during the scratching a pressure is exerted upon the surface; and if even it be assumed that the individual scratches are not pressed in but are scooped

out, in this scooping a pressure is exerted upon the adjacent parts, which, even though exerted laterally, must necessitate a condensation.

Melloni\* also made the following experiment. From a sheet of plate glass 11 millims. in thickness he had four pieces cut; they were heated to redness, and two were allowed to cool slowly, while the others were cooled rapidly. One of each of the two kinds was scratched, and then the four sides of a box were made of the plates, which was filled with hot water. The two slowly cooled ones exhibited the same radiation. Of the quickly cooled ones, the scratched one gave a deflection of  $29^{\circ}7'$ , and the other of  $28^{\circ}$ . Thence Melloni concludes that the roughness of the surface only exerts an influence if the interior of the mass has a smaller density than the radiating layers on the surface.

Knoblauch† subsequently made experiments to confirm in another manner Melloni's view. He used cast and rolled lead plates. One of the first, the cast ones, which when smooth produced a deflection of  $49^{\circ}$ , exhibited after being scratched only a deflection of  $48^{\circ}25'$ . Hence the radiation had decreased—it is true, only by  $0^{\circ}75'$ ; but by a subsequent scratching in a transverse direction it decreased by another degree, for the deflection only amounted to  $47^{\circ}25'$ .

Of the rolled plates, one, which when smooth had produced a deflection of  $50^{\circ}5'$ , after scratching also indicated a decrease in the radiation, for the deflection amounted only to  $48^{\circ}5'$ . After being again scratched in a transverse direction there was again an increase in the radiation of  $1^{\circ}5'$ ; for the deflection amounted to  $49^{\circ}75'$ . M. Knoblauch supposes that the increase in the radiation by the second scratching might be due to the lead being compressed in the positions of the marks, but loosened at the points at which the upturned edges of the furrows intersected.

Knoblauch then took a cube of rolled copper plate, and first compared the radiation of a smooth side with one scratched in two directions. The first produced a deflection of  $29^{\circ}$ , the other of  $47^{\circ}75'$ . Afterwards, both having been covered galvanoplastically with copper, the first gave  $49^{\circ}25'$ , the grooved one  $51^{\circ}5'$ .

This experiment is indeed very interesting; but in my opinion it does not, as Knoblauch maintains‡, confirm Melloni's principle "that scratching the surface only influences the radiating-power of bodies so far as it modifies their density and hardness, and it increases or diminishes the radiating-power accordingly as it loosens or condenses the parts affected;" for M.

\* *Thermocrosc.* p. 88, note.

† *Pogg. Ann.* vol. lxx. p. 343.

‡ *Loc. cit.* p. 343.

Knoblauch himself mentions that, by scratching, the parts under the strokes proper are condensed, but the upturned edges are loosened. Melloni maintains, on the contrary, that scratching does not loosen the part, but only lays bare the less dense interior.

Under these circumstances it seemed desirable to examine the phenomenon a little more minutely; and with this view the author experimented with platinum plates instead of copper and other easily oxidizable metals, so that other alterations also on the surface, such as are produced on silver by small quantities of sulphuretted hydrogen, were not to be feared.

A platinum plate which had been made as hard as possible by rolling, after having been strongly heated, radiated as much heat as before. Here the hardness cannot have affected the radiation.

Another platinum plate had been passed under very great pressure between two rollers, one of which was finely grooved; so that after this treatment one of the sides exhibited small elevations, while the other remained smooth. The first radiated slightly more than the other; but after the plate had been strongly heated, even this difference was no longer perceptible. Hence it follows that, when the surface is otherwise the same, inequalities, and even regularly alternating elevations and depressions, may exist without any increase in the radiation.

When, on the contrary, a plane platinum plate, which had been heated by a glassblower's lamp and was quite soft, was roughened by means of fine emery-paper, the radiation was doubled.

In order to be able to make the comparison, the radiating plate was heated by a small brass apparatus which was kept at  $100^{\circ}$  C. by means of steam. It consisted of a horizontal cylinder of 50 millims. internal diameter and of the same length; the plate under investigation formed one of the ends. In order readily to exchange this plate for another, the cylinder was provided with a broad welt, against which the plate was pressed by means of a brass ring and three screws. In order to make the whole apparatus quite tight, rings of stout paper were used as washers, and rendered it perfectly steamtight.

To be certain that in the treatment of the plate no foreign substance had been left upon it (traces, for instance, of the glue of the emery-paper, although it was used quite dry), the plates before being fastened in the apparatus were heated for a time in concentrated nitric acid, then washed with distilled water until all traces of acid were removed, and then dried without being touched by a cloth or any other object.

It is difficult to conceive that by the slight treatment with emery-paper the density of the surface was so far increased that the radiation was doubled.

When a platinum plate was covered with a thin layer of spongy platinum, by spreading a thin layer of ammoniochloride of platinum upon it and then strongly heating, without treatment with nitric acid, it indicated seven times as much radiation as before being treated with spongy platinum. Spongy platinum is looser than the plate upon which it adheres; but each individual particle is undoubtedly as hard as a particle of the heated plate. Hence the action of the spongy platinum is due to its presenting more points and corners. This is more probable from the fact that the radiation of a plate covered with spongy platinum diminishes when it is frequently and continuously heated. It is possible that at each fresh heating some of the sponge is detached; but in any case the extreme points and corners are rounded off. Harder they cannot become.

The author is of opinion that the increase in the radiation with a roughened surface depends essentially on the refraction which the heat experiences on its emergence from the surface of the radiating body. He explains this influence for various forms of the surface, and arrives thereby at the following conclusion. The greater the refractive index of heat between the radiating substance and air, the smaller is the radiation from the plane surface, and then the quantity of heat reflected inwards increases. The metals have doubtless a very high refractive index. Hence they reflect the rays from without, and allow but few to penetrate; and hence they reflect internally those coming from the interior, and allow but few to emerge. Great inequalities of the radiating surface do not occasion any important alteration in the radiation. Such a one only occurs when the radii of curvature are very small, and change greatly, and when the radiating surface has but little diathermancy. In general, the roughness of the surface may effect both an increase and a diminution in the radiation; but if the inequalities are very fine and very deep, there is almost always an increase in the case of substances which are but slightly diathermanous, as the metals. When there is a very fine powder of the same substance on the radiating surface, the radiation is considerably increased, not merely from substances which, like the metals, are but little diathermanous, but also from very diathermanous ones, such as rock-salt.



LXVI. *Proceedings of Learned Societies.*

## ROYAL SOCIETY.

[Continued from p. 383.]

Feb. 10, 1870.—Lieut.-General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communications were read :—

“On some remarkable Spectra of Compounds of Zirconia and the Oxides of Uranium.” By H. C. Sorby, F.R.S.

When a scientific man has been led into an error and afterwards discovers his mistake, I think it a matter of duty that he should take an early opportunity to correct it. I therefore now write the following notice of certain remarkable peculiarities in the spectra of some compounds of the oxides of uranium with zirconia which led both myself and others\* to conclude that they were due to a new elementary substance.

Though the spectra of the different salts of those bases which show well-marked absorption-bands often differ in detail, yet they usually resemble each other so much that there is no difficulty in recognizing each particular element. This is so constantly the case in the various compounds of erbium, didymium, and cobalt, and in the ordinary salts of uranium, that for a long time the more I studied this question, the more did it appear to be a general rule, and there seemed to be no reason to suspect that a few special compounds of uranium would give spectra with absorption-bands as unlike as possible those of all others. Such, however, turns out to be the fact when its oxides are combined with zirconia.

As an excellent illustration of important differences in mere detail, but general correspondence, I would refer to the spectra of didymium in different states of combination†, and would especially refer to the most distinct of the numerous absorption-bands, which occurs in the yellow. The various compounds agree in showing this band in the same general position; but by careful management, and by the use of sufficient dispersive power, it may be resolved into a very variable number of narrow bands or black lines. For example, in the case of the crystallized sulphate containing comparatively little lanthanum it can be resolved into seven narrow lines, two of those near the centre being the darkest; whereas when much lanthanum is present, one line on the side next the green is so much darker than the rest that the others are comparatively absent. On fusing the mixed oxides with borax, the same spectrum is seen as with oxide of didymium alone, and I can resolve the above-named band into only two narrower bands; whereas when the saturated bead is made to deposit crystals by being kept some time at a very dull red heat, this band can easily be resolved into eight equal and very distinct black lines. Although these and similar differences in detail are of much interest,

\* Professor Church, ‘Chemical News,’ vol. xix. p. 121, and Professor Loew, *ib.* vol. xx. p. 9.

† See also Bunsen’s paper, *Phil. Mag.* Sept. 1866, p. 177.

yet in no case are they so considerable as to prevent our recognizing at once that the spectra are all due to didymium. It is also important to notice that the amount requisite to give a most splendid spectrum when the bead is crystalline, will scarcely show any trace of bands when it is in a vitreous condition, dissolved in the borax. This is analogous to what occurs in the case of solid and powdered crystals of sulphate of didymium; for the absorption-bands in the spectrum of the light transmitted by a thin layer of the fine powder, strongly illuminated from the other side, are as distinct as in that transmitted by a many times greater thickness of solid and transparent crystal. We may very conveniently take advantage of this fact in studying the spectra of such substances when the amount of material at command is otherwise too small. This seems to be because the transmitted light does not simply pass through the crystals, but is in great measure reflected from them backwards and forwards, and thus, as it were, passes through a greater thickness. It is also to a considerable extent similar to that reflected from the powder when illuminated from above, as may be clearly proved by what occurs in the case of uranic salts. These when in a state of moderately fine powder transmit light, giving a spectrum showing not only the absorption-bands in the blue, which alone are met with in that transmitted by a clear crystal, but also the bands in the green, which depend on fluorescence, characteristic of that reflected from the powder\*. These two kinds of bands can be easily distinguished by means of a plate of deep-blue cobalt glass, which has an entirely different action accordingly as it is placed below or above the object when the bands are due to fluorescence, but has no such effect when they are due to ordinary absorption. It would perhaps be well to mention here that I have in this manner proved that the abnormal bands seen in the spectra of the compounds of zirconia with the oxides of uranium, described in this paper, are due to genuine absorption, and not to fluorescence.

The remarkable spectrum of some jargons has been already described by me in the 'Chemical News'†, and in the Proceedings of the Royal Society‡. One of its most striking peculiarities is, that when light passes in a direction perpendicular to the principal axis of the crystal, and the spectrum is divided by means of a double-image prism into two spectra having the light polarized in opposite planes, though some of the absorption-bands are of equal intensity in both images, yet others are comparatively absent, some in one and some in the other; whereas in the case of other dichroic crystals which give spectra with absorption-bands, they are usually all more distinct in one image than when the light is not polarized, and all fainter, or even comparatively absent, in the other. No sooner had I observed this spectrum (No. 5, given below), than I made various experiments in order to ascertain whether uranium was present or not; and the then known tests that could be applied to the amount of material at my command seemed to show that it was absent. This was quite in

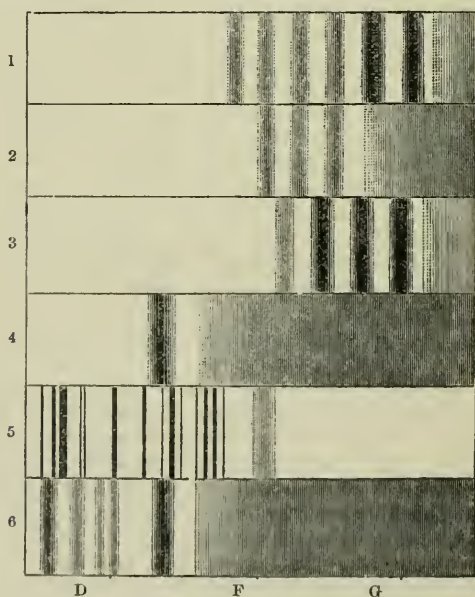
\* See Stokes's papers, Phil. Trans. 1852, p. 463, and 1853, p. 392.

† Vol. xix. p. 122. ‡ Vol. xvii. p. 511. [Phil. Mag. Jan. 1870, p. 65.]

accord with the results of the various analyses published by other chemists, none of whom mention the existence of any trace of that substance. Moreover the general character of the spectrum was entirely unlike that of all the known compounds of uranic oxide. The various artificial salts all agree in giving a variable but small number of moderately broad absorption-bands in the blue end (Nos. 1, 2, and 3); and the same is also seen in the case of several natural minerals; whereas the jargon gave a most unusually large number of narrow black lines (fourteen quite distinct, besides others more faint, and a single broader band which I cannot separate into lines), extending from the red end, so that nearly all occur in that part of the spectrum which is entirely free from bands in all previously known compounds of uranic oxide. This same general fact was also seen in the spectrum (No. 6) of the opaque blowpipe-beads gently flamed, as described in my former paper. These differences will be better understood by means of the following drawing, which shows three of the most striking spectra of uranic salts, that of uranate of soda, and the two which are rendered so abnormal by the presence of zirconia.

Red End.

Blue End.



Spectra of Uranic Compounds.

Nos. 1, 2, and 3. Uranic salts of the common type, viz.:—

1. Native phosphate.
2. Nitrate in crystals.
3. Acetate in crystals.

No. 4. Uranate of soda in the carbonate-of-soda bead.

Nos. 5 and 6. Uranic compounds with zirconia, viz.:—

5. Jargon after ignition.
6. Crystalline borax blowpipe-bead.

I will not now enter into a description of the various chemical and physical facts which seemed to warrant the conclusion that zircons sometimes contain a new earth; but taking these into consideration, there seemed to be every reason to believe that spectra which thus differed so much from those of any previously known substance were characteristic of this new earth. Judging from the facts then known, it was more probable that spectra of such a new type were due to a new element, than that they were due merely to a combination of two such elements as zirconium and uranium. Some of these chemical and physical facts can now be explained by the presence of uranium; but besides this and several of the more common earths and oxides, I have detected in some zircons erbium, didymium, yttria, and another substance which exists in such small quantity that I have not yet been able to ascertain whether or not it is the suspected new earth. These accidental constituents do not, indeed, occur in sufficient quantity to be of importance, except as modifying the physical and optical properties, the didymium giving its usual characteristic absorption-bands (zircons from Sveneroe, Norway), and the manganese the same spectrum as that of garnets (zircons from an unknown locality in Siberia\*). This method, however, fails to give evidence of a new earth; for, since the publication of my former paper, I have proved that the very abnormal spectra which seemed sufficient to establish its existence are really due to compounds of zirconia with the oxides of uranium, which have such a powerful action on light that an almost inappreciable amount is sufficient to produce the spectra to great perfection—in fact, so small an amount that the total quantity which misled me was only a few thousandths of a grain; and its presence might easily have remained unsuspected, if I had not made a number of experiments which at first did not seem to have much connexion with the subject.

In studying the spectra of crystalline blowpipe-beads, it seemed desirable to examine those made with carbonate of soda, with or without a little borax. This when melted dissolves certain oxides; and though it crystallizes on cooling, so as to be only partially translucent, yet with strong direct sunlight well-marked spectra may be seen. For example, in the oxidizing flame uranic oxide is easily dissolved by carbonate of soda alone; and when quickly cooled, an orange-coloured bead is obtained, probably containing uranate of soda in a vitreous condition, which gives a single well-marked absorption-band in the green (see spectrum No. 4); with so small a quantity of the oxide, that in a bead  $\frac{1}{8}$  inch in diameter shows the spectrum to the best advantage, and even  $\frac{1}{10000}$  grain can be easily detected. We need not be surprised that this spectrum differs so much from the usual type of uranic salts (Nos. 1, 2, and 3), since in this case the oxide plays the part of an acid. It may be only an accidental coincidence; but this difference is analogous to the change which commonly occurs on adding an alkali to neutral solutions of vegetable colours†. When gently reheated it seems as if the uranate

\* For both of these I am indebted to my kind friend Mr. David Forbes.

† See my paper in *Proc. Roy. Soc.* 1867, vol. xv. p. 433.



passed into a crystalline state; for the spectrum then shows four absorption-bands, and is more like the ordinary type; but this change does not occur if a little borax has been added. The addition of more and more borax causes the absorption-band to become more and more faint, and to advance towards the blue end, until we obtain a spectrum with very faint bands but of the usual character.

In examining the various products into which I separated jargons in order to study the supposed new earth in a state of purity, I obtained a small quantity of a dark-coloured substance, apparently zirconia containing some oxide which communicated a green tint to a glassy borax blowpipe-bead, but yet not sufficiently distinct to show that it was due to uranous oxide. I therefore thought that the carbonate-of-soda method might throw light on the question; and though the presence of zirconia prevented solution by pure carbonate of soda, the addition of a little borax enabled me to prove that uranic oxide is really present in some jargons. Such, then, being the case, it seemed desirable to ascertain whether the oxides of uranium would give rise to any special spectra when present along with zirconia in crystalline blowpipe-beads. To my astonishment I found that the spectra were precisely the same as those obtained in the case of what I had thought to be an approximately pure new earth\*. When, however, I had ascertained the quantity of oxide requisite to give this result, I was no longer surprised that I had not suspected its presence. In the case of transparent blowpipe-beads of borax with microcosmic salt, it is requisite to have as much as about  $\frac{1}{50}$  grain of uranous oxide to show faintly the characteristic absorption-bands, whereas, when present along with zirconia in the crystalline beads,  $\frac{1}{50000}$  grain gives an equally well-marked spectrum; and  $\frac{1}{2000}$  grain shows it far better than a larger quantity, which makes the beads too opaque. These very minute quantities were obtained by the repeated division of a small known weight, either before or after fusion with borax. This spectrum also differs very considerably from the spectra of the usual salts or blowpipe-beads of uranous oxide. On comparing them side by side, the only common peculiarity is the fact of there being numerous absorption-bands distributed over a large part of the spectrum; but they do not correspond either in number or position. On the contrary, they differ almost as much as possible, and the darker bands in the spectrum of this zirconia compound occur where the transmitted light is the brightest in other cases.

The oxide of uranium is so easily reduced at a high temperature to the state of protoxide in a borax-bead, with excess of boric acid, and is so readily peroxidized at a dull-red heat when crystallized along with borate of zirconia, that there seemed good reason to refer the change in the spectra to temperature rather than to the state of oxidation, until after it was found that they were due to uraninm. By gently flaming the crystalline bead, the spectrum is entirely altered, and becomes like No. 6, which seems to be characteristic of a compound of borate of zirconia with uranic oxide. This gives a spec-

\* Figs. 1 and 2 of my former paper, at p. 67 of the present volume.

trum with five well-marked absorption-bands, all of which occur at the red end, where no trace of bands exists in the case of ordinary salts, as will be seen on comparing it with Nos. 1, 2, and 3. I have tried many experiments in order to ascertain whether any other element besides zirconia will cause uranium to give similar abnormal spectra; but none show any thing of the kind, at all events in similar conditions. A few have special characters, as described below, but the majority exert little or no influence; and even when the blowpipe-beads are crystalline, they show only the usual spectra of the oxides of uranium. Moreover no such great change in the character of the spectra of any other elements which give absorption-bands is to be seen when they are combined with zirconia; and, as far as my present experience goes, it seems as if such very abnormal spectra were met with only in the case of these remarkable compounds of zirconia with the oxides of uranium.

Such, then, being the facts, it appears to me that we are now in a position to explain why certain zircons give three different spectra, as described in my former paper. Some jargons (usually those of a green tint) contain a little uranium so combined that the characteristic spectrum is only faintly visible, whereas, after ignition, the intensity of the absorption-bands is permanently increased to a variable extent, occasionally only a little, but in some cases as much as twenty-five times. This more powerful action on light is accompanied by an increase in hardness and in specific gravity (sometimes as much as from 4.20 to 4.60), as described in my former paper; and I have since found that these changes are approximately proportional to the amount of uranic oxide in the various specimens, as shown by comparing the spectra of the blowpipe-beads. This change may partly depend on the oxidization of the uranous oxide, since some specimens slightly increase in weight when ignited; but I think it cannot be mainly due to that; for sometimes there is no such increase, and uranous oxide combined with zirconia gives rise, not to a spectrum without bands, but to one with several of very marked character, as described below. On the whole, since this abnormal type of spectrum is so characteristic of combination with zirconia, it appears to me more probable that the effect of a high temperature is to cause the uranic oxide to combine more specially with the zirconia, as though the greater part existed naturally as a silicate, but after ignition as a zirconiate. We may also apply the same explanation in the case of zircons more or less strongly coloured by other oxides, which become almost colourless when heated; and thus this unexplained peculiarity of zircons may depend on the fact of zirconia being able to play the part of both a base and an acid which, as compared with silica, has an affinity for bases varying according to the temperature.

The brown-red zircon from Ceylon, named at page 514 of my former paper, kindly presented to me by Mr. E. L. Mitford, of Rusthall, gives a spectrum precisely like that of the borax blowpipe-beads crystallized after treatment in the deoxidizing flame, and therefore no doubt contains uranous oxide. This spectrum being given by

only one part of the crystal, probably depended upon the presence of some substance which either reduced the uranic oxide or prevented the oxidization of the uranous.

These facts thus clearly show that the various spectra which seemed to indicate the presence of a new element existing in three different physical conditions, are in reality only characteristic of the two oxides of uranium combined with zirconia, or not in combination. Perhaps some may think that my having been thus led astray shows that little or no reliance can be placed on the method of investigation employed; but I contend that the mistake was due to its being such an unexpectedly delicate test for uranium; and, as explained above, the error was ultimately corrected by a further development of the same method. As far as the interests of science are concerned, there is no need to regret the general result. We have lost what appeared to be good evidence of a new earth, but have gained an almost entirely new system of blowpipe testing, which enables us to detect such a minute quantity of some substances as could not be recognized by the ordinary means. I shall not now attempt to give any thing like a full account of this subject, since it would be much better to let it form part of a paper on various improvements in blowpipe chemistry, but will merely mention a few facts which have a special bearing on the question before us.

In the first place, I would say that zirconia and the oxides of uranium are most useful reagents in detecting the presence of certain substances with which they unite to form compounds having very special characters. The most striking of these are :—the compounds already described, which are distinguished by the spectra, and not by any well-marked colour; the compound of ceric oxide with uranic oxide, which is of a splendid deep blue colour, but shows no absorption-bands; and that of yttria with uranic oxide, which is characterized by a deep orange-colour and extreme fusibility. Thorina and oxide of lanthanum form with uranous oxide compounds which give spectra with absorption-bands in special positions, but of the usual type, and not of such a marked character as to be useful in detecting minute quantities of those substances in mixtures.

In order to see the spectra of the zirconium-uranium compounds, it is requisite that both elements should be combined in a crystalline condition. When both constituents are melted in borax and are held in solution, or if when crystals are deposited any other substance replaces either the zirconia or the oxides of uranium, the characteristic spectra cannot be seen. The most simple application of this test for uranium is in the case of various zircons. As much of the powdered mineral as will dissolve should be melted with borax in a circular loop of platinum wire about  $\frac{1}{8}$  inch in diameter, so as to give a bead of moderate thickness. A little boric acid should then be added, which not only tends to keep the uranium in the state of protoxide, but also facilitates the crystallization of the borate of zirconia, which is far less soluble when there is excess of boric acid. The bead should then be kept at a bright red heat, just within the deoxidizing flame, until so much borax has been volatilized that small



needle-shaped crystals begin to be deposited, when it must be allowed to cool rapidly. It should then be transparent, with scattered crystals, and the uranium all in the state of protoxide. On gently reheating it, the bead ought to suddenly turn white and almost opaque; and care must be taken not to heat it any more than is just requisite to cause the borate to crystallize out, or else the uranium will rapidly pass into the state of peroxide. Such beads must be examined by strong direct light from the sun, or from a lamp of very great brilliancy, condensed on them by means of an almost hemispherical lens of about  $\frac{1}{2}$  inch focal length; and in addition to the means described in my former paper, I have since found it very convenient to place them over a hole in a black card, so as to entirely prevent the passage of any light which has not penetrated through them, even when so arranged in the focus of the microscope that the spectrum of their thin edges may be examined, if the centre be too thick and opaque. If thus properly prepared, the presence of more or less uranium will be shown by the greater or less intensity of the absorption-bands of the spectrum described and shown in fig. 1 of my former paper. This test is so delicate that there is no difficulty in seeing the darker band in the green in the case of zircons which contain no more than  $\frac{1}{100}$  per cent. of uranic oxide; and I find that very few localities yield this mineral so free from it that it cannot be easily detected. Those from Miask, Siberia, are the only specimens in which I have not been able to recognize it. The jargons from Ceylon contain an amount varying up to about 1 per cent., although in no published analysis that I have seen is there any allusion to the presence of even a trace. It has also been overlooked in several other cases; and it now becomes important, because it gives rise to various well-defined spectra, which are so characteristic of the different minerals, that they can be very conveniently identified, even when cut and mounted as jewels, by means of the number and position of the absorption-bands, as I intend to explain in a paper on the spectra of minerals.

On flaming the bead at a moderate red heat, the protoxide passes into the peroxide, and the spectrum No. 6, given above, may be seen, if sufficient oxide be present; but considerably more is required than in the case of the protoxide. I may here say that the examination of better preparations has enabled me to detect another distinct band in the extreme red, not shown in fig. 2 of my former paper, and also an additional faint band in the blue, not shown in fig. 1.

In applying this test to detect minute quantities of uranium in other minerals, it is requisite to bear in mind that zirconia may play the part of both an acid and a base, and that various oxides and acids so combine with the zirconia or with the oxides of uranium as to prevent the formation of the compounds which give rise to the characteristic spectra. The zirconia appears to combine with some rather than with the uranous oxide, and with others rather than with the uranic, so that, if one spectrum cannot be obtained, the other may; and there are few, if any, cases when neither can be seen, especially if care be taken to use excess of zirconia. If, however, the



amount of uranium be very small, and so much of other oxides be present as to make the bead very dark, or too opaque from deposited crystals, before it is sufficiently concentrated for the compounds with the oxides of uranium to crystallize out, it may be impossible to detect it. In order to apply the test in the case of complex minerals, a bead of borax, boric acid, and pure zirconia should be prepared, then a small quantity of the mineral added, and, after fusion and sufficient concentration, the bead made to crystallize in the manner already described. If needle-shaped crystals be not deposited in the bead when very hot, and if it do not suddenly turn opaque when reheated, the result may not be satisfactory. In this manner it is easy to detect uranium in  $\frac{1}{200}$  grain of such minerals as Fergusonite, tyrite, and ytrotantalite, even when they contain no more than 1 or 2 per cent. If in such cases the spectrum of the uranic compound cannot be obtained, the bead should always be flamed and reexamined, to see if that of the uranic compound is thereby developed.

In a similar manner we may make use of a little oxide of uranium to detect zirconia; but the test is far less delicate than the converse, because it is almost impossible to obtain the compound in a crystalline state, unless there be an excess of zirconia. Not more than  $\frac{1}{1000}$  grain of uranic oxide should be employed, or the bead may be too opaque. There is no difficulty in thus detecting zirconia in zircons, or in katapleite; but the presence of so much of other bases in minerals like eudialyte prevents our obtaining a satisfactory result. There certainly could not be a more characteristic test to confirm the results of other methods, or to identify such a small quantity of approximately pure zirconia as could not easily be distinguished in any other way.

The only other compound of uranic oxide of very abnormal character which I have so far discovered is that with ceric oxide. So much of both oxides should be fused with borax in the oxidizing flame as will yield a bead which is perfectly clear, and of pale yellow colour when rapidly cooled, but crystallizes when gently reheated. If the constituents be present in a certain proportion, it then turns from pale yellow to a deep blue, as though coloured by oxide of cobalt. In most cases the bead is rendered nearly opaque by the number of crystals; but sometimes, though it turns deep blue, it remains transparent, owing to the compound being set free in a state similar to that of the red oxide of copper in a borax blowpipe-bead, with carbonate of soda and oxide of tin, treated in the reducing flame. The spectrum of these blue beads shows no absorption-bands, but merely a general absorption at the red end; and it is curious to find that the combination of two yellow substances gives rise to a deep blue, in much the same manner as when the yellow ferrocyanide of potassium is added to a yellow ferric salt. The production of this blue colour on the addition of a little uranic oxide might be employed with advantage to identify moderately small quantities of cerium, even when mixed with a number of other substances; but unfortunately the presence of much oxide of lanthanum, which is so commonly associated with it,

interferes, as though the ceric oxide had a stronger affinity for the oxide of lanthanum than for uranic oxide.

The most characteristic peculiarity of the compound of yttria and uranic oxide is that it will not crystallize out from a borax blowpipe-bead, and that the affinity of the uranic oxide for yttria is stronger than for zirconia. Perhaps erbia may prove to act in the same way; but I have not been able to examine that earth quite free from yttria. On adding yttria to a bead with zirconia and a little uranic oxide, and gently flaming it in the oxidizing flame, the uranic oxide combines with the yttria and rises to the surface as an orange-coloured scum, which has a great tendency to collect on the platinum wire; and if sufficient yttria be added, the crystallized borate of zirconia is left in the interior almost colourless, and so free from uranic oxide that no absorption-bands can be seen in the spectrum. We may take advantage of this circumstance to detect yttria in small quantities of compound minerals like Gadolinite and Fergusonite; and I may here say that by combining such means with the observation of the spectra of the transparent or crystalline beads, and of the form of the crystals when slowly deposited, with or without the addition of suitable reagents\*, we may often detect twice as many constituents in minerals as could be detected by the ordinary methods of blowpipe chemistry—an advantage which, I am sure, will be appreciated by those engaged in the study of rocks, when it is often so important to obtain satisfactory results with small quantities of material. I have also found these methods of great practical use in examining small residues in the qualitative analysis of minerals, and have thus unexpectedly discovered small quantities of comparatively rare elements.

I have tried the effect of many other substances along with zirconia and the oxides of uranium, and find that most of them have no sensible influence, unless they are present in considerable relative quantity. The most striking effect is that of oxide of tin, which causes the two absorption-bands in the yellow and yellow end of the green in the spectrum of the uranous oxide compound to be nearly equally dark, whereas without the oxide of tin that in the yellow is comparatively faint. This is another illustration of the manner in which certain substances, having no special action on light, influence by their presence the properties of another. The oxides of uranium are unusually sensitive to such actions, and thus not only lend themselves to us as blowpipe-reagents, but also seem more than any others to afford the means of explaining the relation between the physical conditions of compounds and their action on light.

The only compound of zirconia with any other oxide to which I need now draw attention is that with chromic oxide as deposited from a borax blowpipe-bead. After treatment in the deoxidizing flame, when the cooled very pale green bead is gently reheated, this compound crystallizes out so as to give a fine red-pink colour by transmitted light, even when so little chromium is present that the glassy bead is scarcely at all green. If too strongly heated, the pink tint is lost. This compound is of interest in connexion with the

\* See my paper, *Monthly Microscopical Journal*, vol. i. p. 349.

colour of rubies and other minerals coloured red by chromic oxide. To others, like the emerald, it imparts a green colour; and, on the whole, it acts on light in such a variable manner, according to the presence of other substances, that the spectra may be made use of as a means of identifying particular minerals, though they do not present any thing like such striking anomalies as these met with in the compounds of zirconia with the oxides of uranium.

“On the Mathematical Theory of Stream-lines, especially those with four Foci and upwards.” By William John Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edinb., &c.

A *Stream-line* is the line that is traced by a particle in a current of fluid. In a steady current each individual stream-line preserves its figure and position unchanged, and marks the track of a filament or continuous series of particles that follow each other. The motions in different parts of a steady current may be represented to the eye and to the mind by means of a group of stream-lines.

Stream-lines are important in connexion with naval architecture; for the curves which the particles of water describe relatively to a ship, in moving past her, are stream-lines; and if the figure of a ship is such that the particles of water glide smoothly over her skin, that figure is a *stream-line surface*, being a surface which contains an indefinite number of stream-lines.

The author in a previous paper proposed to call such stream-lines *Neoids*; that is, ship-shape lines.

The author refers to previous investigations relating to stream-lines, and especially to those of Mr. Stokes, in the Cambridge Transactions for 1842 and 1850, on the “Motion of a Liquid past a Solid,” and of Dr. Hoppe, on the “Stream-lines generated by a Sphere,” in the Quarterly Journal of Mathematics for 1856, and to his own previous papers on “Plane Water-lines in Two Dimensions,” in the Philosophical Transactions for 1864, and on “Stream-lines,” in the Philosophical Magazine for that year. He states that all the neoid or ship-shape stream-lines whose properties have hitherto been investigated in detail are either *unifocal* or *bifocal*; that is to say, they may be conceived to be generated by the combination of a uniform progressive motion with another motion consisting in a divergence of the particles from a certain point or focus, followed by a convergence either towards the same point or towards a second point. Those which are continuous closed curves when unifocal are circular, and when bifocal are blunt-ended ovals, in which the length may exceed the breadth in any given proportions. To obtain a unifocal or bifocal neoid resembling a longitudinal line of a ship with sharp ends, it is necessary to take a part only of a stream-line; and then there is discontinuity of form and of motion at each of the two ends of that line.

The author states that the occasion of the investigation described in the present paper was the communication to him by Mr. William Froude of some results of experiments of his on the resistance of model boats, of lengths ranging from three to twelve feet. A sum-



many of those results is printed at the end of a Report to the British Association on the "State of Existing Knowledge of the Qualities of Ships." In each case two models were compared together of equal displacement and equal length: the water-line of one was a wave-line with fine sharp ends; that of the other had blunt rounded ends, each joined to the midship body by a slightly hollow neck—a form suggested, Mr. Froude states, by the appearance of water-birds when swimming. At low velocities, the resistance of the sharp-ended boat was the smaller; at a certain velocity, bearing a definite relation to the length of the model, the resistances became equal; and at higher velocities the round-ended model had a rapidly increasing advantage over the sharp-ended model.

Hence it appeared to the author to be desirable to investigate the mathematical properties of stream-lines resembling the water-lines of Mr. Froude's bird-like models; and he has found that endless varieties of such forms, all closed curves free from discontinuity of form and of motion, may be obtained by using *four* foci instead of two. They may be called, from this property, *quadrifocal stream-lines*, or, from the idea that suggested such shapes to Mr. Froude, *cycnoids*; that is, swan-like lines\*.

Those lines are not to be confounded with the lines of a yacht having at a distance the appearance of a swan, which was designed and built some years ago by Mr. Peacock, for the figure of that vessel is simply oval.

The paper contains four chapters. The first three are mainly cinemactical and geometrical, and relate to the forms of stream-line surfaces in two and in three dimensions, especially those with more than one pair of foci and surfaces of revolution, to the methods of constructing graphically and without calculation, by means of processes first applied to lines of magnetic force by Mr. Clerk Maxwell, the traces of such surfaces, which methods are exemplified by diagrams drawn to scale, and to the motions of the particles of liquid past those surfaces. The fourth chapter is dynamical; it treats of:—the momentum and of the energy of the disturbance in the liquid, caused by the progressive motion of a solid that is bounded by a ship-shape stream-line surface of any figure whatsoever; of the ratio borne by the total energy of the disturbance in the liquid to that of the disturbing body when that body displaces a mass of liquid equal to its own mass, which ratio ranges in different cases from  $\frac{1}{2}$  to 1; of the acceleration and retardation of ships as affected by the disturbance in the water, and especially of the use of experiments on the retardation of ships in finding their resistance; and of the disturbances of the pressure which accompany the disturbances of motion in the liquid. Up to this point the dynamical principles arrived at in the fourth chapter are certain and exact, like the geometrical and cinematic principles in the three preceding chapters. The results obtained in the remainder of the fourth chapter are in some respects approximate and conjectural, and are to a great extent designed to suggest plans for future experiments, and rules for their

\* Κυκνοειδής.



reduction. These results relate to the disturbances of level which accompany the disturbances of motion when the liquid has a free upper surface, to the waves which originate in those disturbances of level, and the action of those waves in dispersing energy and so causing resistance to the motion of the vessel, to friction, or skin-resistance, and the "wake" or following current which that kind of resistance causes the disturbing solid body to drag behind it, and to the action of propelling instruments in overcoming different kinds of resistance.

The resistance caused by viscosity is not treated of, because its laws have been completely investigated by Mr. Stokes, and because for bodies of the size of ships, and moving at their ordinary velocities, that kind of resistance is inconsiderable compared with skin-resistance and wave-resistance. The resistance caused by discontinuity of figure is stated to be analogous in its effects to friction; but it is not investigated in detail, because ships ought not to be built of discontinuous (commonly called "unfair") figures.

#### Supplement.

The author in the first place calls attention to the agreement between the position of the points at which there is no disturbance of the pressure on the surface of a sphere, as deduced from Dr. Hoppe's investigation, published in 1856 (*Quarterly Journal of Mathematics*), and on the surface of a short vertical cylinder with a flat bottom, as determined by the experiments of the Rev. E. L. Berthon before 1850 (*Proc. Roy. Soc.* vol. v. 1850; also *Transactions of the Society of Engineers*, 6th of December, 1869). The theoretical value of the angular distance of those points from the foremost pole of the sphere is  $\sin^{-1} \frac{2}{3} = 41^{\circ} 49'$ ; the value deduced from experiment is  $41^{\circ} 30'$ .

The author then adds some remarks on a suggestion made by Mr. William Froude, that the wave-resistance of a ship is diminished when two series of waves originating at different points of her surface partially neutralize each other by interference; and states that, with regard to this and many other questions of the resistance of vessels, a great advancement of knowledge is to be expected from the publication in detail of the results of experiments on which Mr. Froude has long been engaged.

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#### GEOLOGICAL SOCIETY.

[Continued from p. 392.]

November 10th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S.,  
President, in the Chair.

The following communications were read:—

1. "Australian Mesozoic Geology and Palæontology." By Charles Moore, Esq., F.G.S.

The author referred to the observations of Professor M'Coy and the Rev. W. B. Clark on the occurrence of fossils of Mesozoic age

in Australia, and then proceeded to notice the species which he had obtained from that region. Fossils of Mesozoic type occur in Western Australia, in the centre of the continent on Stuart's route, and in Queensland; but the specimens have hitherto been found in apparently drifted blocks, and nothing is known of the bedded rocks from which they are derived. The author stated that the Australian Mesozoic fossils agree, not only in genera, but also in many cases in species, with British forms; and he gave a list of species from Western Australia, identical with British species from the Middle and Upper Lias, the Inferior Oolite, and the Cornbrash. Of the fossils from Queensland also, many are said to be identical with, or very nearly allied to, British species; but the author regards the general type of the Queensland remains as referring them to the Upper Oolite. A gigantic species of *Crioceras* is regarded by the author as possibly indicative of the occurrence of Neocomian deposits in Australia. The fossil evidence upon which Professor M'Coy inferred the occurrence of the Muschelhalk in Australia was said by the author to be nugatory, his supposed *Myophoria* proving to be a *Trigonia* nearly allied to *T. gibbosa* of the Portland Oolite, and his doubtful *Orthoceras* a small *Serpula*. The author had found no indications of the existence of Triassic or Liassic deposits in Queensland.

The blocks from Western Australia referred by the author to the Middle Lias contain *Myacites liassianus* (Quenst.), and are quite as highly ferruginous as the English Marlstone. The species identified by the author with British Oolitic species would indicate a range from the Inferior Oolite to the Cornbrash; the author suggests that the species may have had a longer range in time in Australia than in England, or that the subordinate divisions of the Oolite were not clearly marked in the Australian Mesozoic deposits. He is inclined to refer the fossils to the period of the Inferior Oolite.

The author inferred, from the occurrence of these Mesozoic fossils in drifted blocks at the two extremities of Australia, separated by 38° of longitude, that an enormous denudation of rocks of the secondary series has taken place over a considerable part of Australia.

2. "On a Plant- and Insect-bed on the Rocky River, New South Wales." By Charles Moore, Esq., F.G.S.

The organic remains noticed by the author were found by him in a small block of chocolate-coloured, micaceous, laminated marl, obtained from a bed about ten feet thick, at a depth of 100-110 feet, in the auriferous drifts of Sydney flats, on the banks of the Rocky River. The author found the leaves of two forms of Dicotyledonous plants, fragments of a flat narrow leaf which he refers to the Coniferæ, a seed-vessel, and the impressions of several seeds. The insect-remains consist principally of the elytra of Beetles, among which Buprestidæ appear to predominate. The vegetable-remains seem to indicate that the deposit is of Tertiary age.

LXVII. *Intelligence and Miscellaneous Articles.*

ON THE INEXPEDIENCY OF ERRONEOUS HYPOTHESES.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

THE following remarks on the inexpediency of provisionally adopting in Science hypotheses avowedly erroneous were suggested by a recent lecture of a learned Professor whose authority in scientific matters is justly entitled to great weight. While freely admitting the probably erroneous nature of the hypothesis, he suggested the adoption of the "two-fluid" theory of electricity, on the score of its *convenience* in the explanation of electrical phenomena—rejecting, however, with some little inconsistency, the qualitative terms "vitreous" and "resinous," and adopting the merely quantitative adjuncts "positive" and "negative," which are essentially associated with the equally erroneous "one-fluid" theory.

Even the convenience of this hypothesis is not very obvious; for although the idea of an electric "fluid" flowing over a conductor is not at first sight inappropriate, still, that a fluid should travel at the rate of 250,000 miles in a second (the velocity of electric transmission in a copper wire as approximately determined by Sir C. Wheatstone) does great violence to our conceptions of fluid-motion.

Might not the nature of electricity be as intelligibly, and with much greater verisimilitude, explained to a partially scientific audience thus:—

If any two substances, or two portions of the same substance, be energetically rubbed against each other, a molecular agitation or disturbance of the particles is set up, which is recognized as *heat*. But if certain substances be rubbed against each other, a molecular agitation, differing in kind from the former in some manner at present unknown, is produced in the two substances, conferring upon them certain physical properties, known as *electricity*, which, being in some respects of a mutually opposite character, have, for the sake of distinction, been termed positive and negative. Thus, if a glass tube and a pad of silk, a stick of sealing-wax and a pad of flannel, be respectively rubbed against each other, then the glass and the flannel, the sealing-wax and the silk, respectively manifest similar properties; the former are said to be positively, the latter negatively electrified; and so forth, theory being further dispensed with in the exposition of electrical phenomena.

Nor is this hypothesis at all improbable, as there are some grounds for believing that the friction that develops electricity does not develop heat, although this has not yet been fully demonstrated. Moreover, the substitution of heat for electricity and of electricity for heat at the point of junction of two dissimilar metals was some time since shown by the writer; and the evolution of light and heat at a point of great resistance interposed in a voltaic circuit, as in the electric lamp, are strong arguments in favour of the dynamic, in contradistinction to the material, theory of electricity.

The mind is proverbially tenacious of first impressions ; and the chief idea that an imperfectly scientific audience would have carried away from the lecture in question, is that electricity is a form or kind of matter, and not a mode of motion, or phase of dynamic energy.

I am, Gentlemen,

Yours faithfully,

16 Fitzroy Square, May 23, 1870.

CHARLES BROOKE.

RESULTS OF A RESEARCH ON THE ABSORPTION-SPECTRUM OF  
IODINE-VAPOUR. BY ROBERT THALÉN\*.

From my investigations on the absorption of iodine-vapour I have arrived at the following results†:—

1. The dark bands on the absorption-spectrum of iodine do not extend the entire length of the visible spectrum, but only to the half between the red and green parts.

2. When the absorption has reached its maximum (that is, when the absorbed part of the spectrum forms almost a single continuous band) *the violet part still remains without the smallest change*; hence originates the violet colour characteristic of iodine-vapour.

3. The successive bands do not form a single series, but *several* mixed with each other—a fact which may be very distinctly recognized in the periodical changes which the intensity of the bands presents.

4. The bands belonging to a given series are not *equidistant*; but their mutual distances increase progressively with the wave-lengths, though they are not proportional to them.

5. Each band *may be resolved into several very fine lines* which form among themselves more or less regular groups.

ON A SIMPLE METHOD OF DISPENSING WITH OBSERVATIONS OF  
TEMPERATURE AND PRESSURE IN GAS-ANALYSES. BY WOLCOTT  
GIBBS, M.D.‡

In absolute determinations of nitrogen and other gases, accurate observations of temperature and pressure are, in the ordinary methods of analysis, necessary, and when made require subsequent calculations which, when the analyses are numerous, become rather tedious. By the following simple method these observations may be altogether dispensed with, and the true weight or the reduced volume of the

\* From an abstract communicated by the author to Poggendorff's *Annalen*—the complete paper, with three large copperplates, having appeared in the *Kongl. Svenska Vetensk. Acad. Handl.* for 1869.

† In all these investigations I made use of six flint-glass prisms of 60°; and the spectroscope was that which I used both for drawing the violet part between G and H of the solar spectrum ("On the Fraunhofer Lines, together with a Diagram of the Violet parts of the Solar Spectrum," by A. J. Ångström and R. Thalén, *K. Vet. Acad. Handl.* 1865), and in the determination of the wave-lengths of the metal lines ("Mém. sur la détermination des longueurs d'onde des raies métalliques," par R. Thalén, *Nova Acta Soc. Sc. Ups.* vol. vi. 1868).

‡ Read before the National Academy of Sciences, September 1869.  
*Phil. Mag.* S. 4. Vol. 39. No. 263. June 1870. 2 H



observed gas obtained at once by a single arithmetical operation. The volume of any gas at the temperature  $0^\circ$  C. and pressure 760 millims. may be deduced from its volume at the temperature  $t$  and pressure  $p$  by the familiar expression

$$V_0 = V_1 \frac{1}{1 + 0.00367t} \cdot \frac{h - h' - h''}{760}; \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which  $h$  is the observed height of the barometer (reduced to  $0^\circ$  C.),  $h'$  the tension of the vapour of water at  $t^\circ$  when the gas is moist, and  $h''$  the height of the column of mercury in the collecting-tube above the level of the mercury in the cistern. For any other gas under precisely the same circumstances of temperature and pressure, we have the equation

$$V'_0 = V'_1 \frac{1}{1 + 0.00367t} \cdot \frac{h - h' - h''}{760}. \quad . \quad . \quad . \quad . \quad (2)$$

Whence, dividing the first equation by the second, we have

$$\frac{V_0}{V'_0} = \frac{V_1}{V'_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

or, as a proportion,

$$V_1 : V'_1 :: V_0 : V'_0; \quad . \quad . \quad . \quad , \quad . \quad . \quad . \quad . \quad (4)$$

from which it appears that the reduced volume (volume at  $0^\circ$  and 760 millims.) of the second gas may be found without observations of temperature and pressure, provided that the unreduced volume be observed under the same circumstances of temperature and pressure as the volume of the first gas, the reduced volume of which has been previously determined. Let the first or standard gas be air; then if the weight of one cubic centimetre of dry air at  $0^\circ$  and 760 millims. be  $w$ , the whole weight will be  $wV_0$ . In like manner we shall have for the weight of the gas to be measured  $w_1V'_0$ ; and since the weights do not change with the temperature and pressure, we have, finally,

$$wV_1 : w_1V'_1 :: wV_0 : w_1V'_0.$$

If now we suppose that the gas in the first tube, or standard gas, is, for example, nitrogen, the volume remaining the same, and that the gas to be measured is also nitrogen, we have

$$w_1V_1 : w_1V'_0 :: w_1V_0 : w_1V'_0,$$

or simply

$$V_1 : V'_1 :: w_1V_0 : w_1V'_0. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The application of this formula in practice is as follows:—A graduated tube holding about 150 cubic centims. is filled with mercury and inverted into a mercury-trough. Two-thirds or three-fourths of the mercury are then displaced by air, care being taken to allow the walls of the tube to be slightly moist so as to saturate the air. This tube may be called the companion-tube; the volume of air which it contains must be carefully determined in the usual manner by five or six separate observations, taking into account, of course, all the circumstances of temperature and pressure. The mean of the reduced volumes is then to be found, and forms the constant quantity  $V_0$ . The gas to be measured is transferred from the receiver in which it is collected into a (moist) eudiometer-tube, which is then suspended

by the side of the companion-tube and in the same trough or cistern. Both tubes being supported by cords passing over pulleys, it is easy to bring the level of the mercury in the two tubes to an exact coincidence. The pressure on the gas is then the same in each tube. The temperature is also the same, as the tubes hang side by side in the room set apart for gas-analyses, and are equally affected by any thermometric change. It is then only necessary to read off the volumes of the gas in the two tubes to have all the data necessary for calculating the weight of the gas to be measured. This calculation may be effected in two ways, each of which will be found of use. Thus proportion (4) reads in words:—as the observed volume of the air in the companion-tube is to the observed volume of the gas in the measuring-tube, so is the reduced volume of the air in the first (previously determined as above) to the reduced volume of the gas to be measured. This method, of course, applies to the reduction of any gaseous mixture whatever to the normal pressure and temperature. In absolute nitrogen determinations, however, proportion (5) gives the weight of the nitrogen measured at once, since the term  $w_1 V_0$  is found by multiplying the weight of 1 cubic centim. of nitrogen at  $0^\circ$  and 760 millims. by the reduced volume of air in the companion-tube, and is a constant which can be used as long as the companion-tube lasts. In practice, a companion-tube filled with mercury will last with a little care for a very long time. Even when filled with water I have found that excellent results may be obtained, and that the tube will last for some weeks. Williamson and Russell, in their processes for gas-analysis, have employed a companion-tube for bringing a gas to be measured to a constant pressure; but the application made above is, I believe, wholly new.—*Silliman's American Journal* for May 1870.

#### PHYSICAL NOTICES. BY J. MÜLLER.

I. In the 136th volume of these *Annalen* I have described a method for determining, by means of a vibration-chronoscope, *the time which elapses between the commencement of a flash of lightning and a motion of the hand made in consequence of perceiving it*. In the case of one young man, whom I will denote by M, I had found this interval to be 0.145 of a second.

I have recently repeated these experiments in the case of two of my pupils, and have found that this interval amounted in one case to 0.183 and in another to 0.165 of a second.

II. During the late frost I incited one of my pupils to make experiments to determine the *coefficient of the friction of iron upon ice*. The young man remained standing on his skates (which were not grooved) holding in his hand a *spring balance*. To this a stout cord was fixed by which a second skater drew him along the ice. At the commencement the balance indicated a pull of from 10 to 12 pounds; during the course it varied between 2 and 4 pounds. As the subject of the experiment weighed 125 pounds, the coefficient of friction in question amounted at the commencement to 8–9 per cent. of the burden, but during the motion only to 1.6–3.2 per cent.—*Pogendorff's Annalen*, March 1870.

Freiburg im Br, January 1870.

## INDEX TO VOL. XXXIX.

---

- ABBOTT (Rev. T. K.) on some propositions in the theory of the tides, 49.
- Acetic ether, on the successive action of sodium and iodide of ethyle upon, 303, 394.
- Acids, on the action of the alkali-metals on the ethers of the, 286.
- Affinity, on Berthollet's theory of, 410.
- Air, on the organic dust of the, 229.
- Aldis (T. S.) on Goodricke's theory of algal, 363.
- Algal, on Goodricke's theory of, 363.
- Andrews (Dr. T.) on the continuity of the gaseous and liquid states of matter, 150.
- Arcturus, on the heating-power of, 376.
- Atomic theory, remarks on the, 122.  
— volume of solid substances, on the, 339.
- Auroral appearances and their connexion with the phenomena of terrestrial magnetism, on, 159.
- Avogadro's law, on the deduction of, from the fundamental conception of the mechanical theory of gases, 317.
- Ball (Prof. R. S.) on an elementary proof of a theorem of Lagrange's, 107.
- Barometer, description of a new portable mercurial, 371.
- Battery, on a new secondary, 52.
- Berthollet's theory of affinity, observations on, 410.
- Bezold (Prof. von) on a new kind of electrical dust-figures, 392.
- Books, new:—Edwards's Domestic Fireplaces, 300; Mayer's Lecture-notes on Physics, 302; Dexter's Theory of Existence, *ib.*
- Branley (E.) on solar radiation, 315.
- Brooke (C.) on the inexpediency of erroneous hypotheses, 464.
- Cable-testing, observations on, 169.
- Calantarients (J. A.) on a new portable mercurial barometer, 371.
- Challis (Rev. Prof.) on a mathematical theory of the tides, 18, 260, 435.
- Chemical statics, researches on, 410.
- Chemistry, on the province of mineral, 348.
- Circuit, on the size of the wire which, wound upon a galvanometer or electromagnet, will produce the maximum magnetic effect in a, 109.
- Clarke (Capt. A. R.) on the course of geodesic lines on the earth's surface, 352.
- Climate, on ocean-currents in relation to the physical theory of secular changes of, 81, 180.
- Cockle (Sir J.) on criticoids, 201.
- Comets, observations on, 233; on a new theory of, 401.
- Corona, observations of the, during the total eclipse, Aug. 7, 1869, 17.

- Cretaceous and Jurassic periods, on the limits of the, 321.
- Criticoids, on, 201.
- Croll (J.) on ocean-currents, 81, 180.
- Currents, on secondary, 52.
- Curves, on the mechanical description of, 304.
- Davis (A. S.) on a theory of nebulae and comets, 401.
- De la Rive (Prof. A.) on the organic dust of the air, 229.
- Desains (P.) on solar radiation, 315.
- Dewar (J.) on the atomic volume of solid substances, 339; on inverted sugar, 345.
- Douglas's optometer, remarks on, 9.
- Draper (Prof. J. C.) on an improvement in filtration, 335.
- Duppa (B. F.) on the action of sodium and iodide of ethyle upon acetic ether, 309.
- Ear, on the structure of the human, 248.
- Earth, on the determination of the absolute horizontal intensity of the magnetism of the, 114; on the course of geodesic lines on the surface of the, 352.
- Eclipse, total, of Aug. 7, 1869, observations of the corona during the, 17.
- Ekman (Prof. F. L.) on the connexion between the electrical insulating-power and chemical composition of various kinds of glass, 437.
- Electrical dust-figures, on a new kind of, 392.
- Electromagnetic experiment, on an, 428.
- Elements, on the refraction-equivalents of the, 231.
- Elvanites of Knockmahon, on the composition of the, 12.
- Eye, on the structure of the, 10.
- Felsites of Knockmahon, on the composition of the, 12.
- Felspars, on the atomic constitution of the, 348.
- Filtration, on an improvement in, 335.
- Flame-temperatures, investigation of, in their relations to composition and luminosity, 290, 337.
- Fluoride of silver, on the preparation, composition, and physical properties of, 374.
- Frankland (Prof. E.) on the action of sodium and iodide of ethyle upon acetic ether, 309.
- Garbett (E. L.) on popular difficulties in tide theory, 174.
- Gas, on the variation of temperature in a perfect, during expansion and contraction, 288.
- Gas-analyses, on a simple method of dispensing with observations of temperature and pressure in, 465.
- Gases, on the expansion of, 127; on the mechanical theory of, 317; on the theory of the variation of temperature in, 347; on the spectra of some, under high pressures, 365.
- Geodesic lines, on the course of, on the earth's surface, 352.
- Geological Society, proceedings of the, 313, 383, 462.
- Gibbs (Dr. W.) on a simple method of dispensing with observations of temperature and pressure in gas-analyses, 465.
- Gill (R.) on a possible cause of the Gulf-stream, 238.
- Glaciers, on the veined structure of the ice of, 241.
- Gladstone (Dr. J. H.) on the refraction-equivalents of the elements, 231.
- Glass, on the connexion between the electrical insulating-power and chemical composition of various kinds of, 437.
- Goodricke's theory of algal, on, 363.
- Gore (G.) on fluoride of silver, 374.
- Gulf-stream, on the influence of the, on the climate of the arctic regions, 97; on a possible cause of the, 238.
- Guthrie (Prof. Francis) on  $\sqrt{-1}$ , 282.
- Guthrie (Prof. Frederick) on approach caused by vibration, 309.
- Harrison (J. P.) on solar radiation in connexion with cloud and vapour, 70, 299.
- Heat, on the passage of radiant, through ice, 8; on ocean-currents in relation to the distribution of, over the globe, 81; on the polarization of, 280; on the diathermancy of a series of substances for obscure, 396; on the change in the radiation of, by roughness of the surface, 445.
- Henth (Rev. J. M.) on the circumstances which determine the varia-



- tion of temperature in a perfect gas during expansion and contraction, 288, 347; on thermodynamics, 421.
- How's (Prof.) contributions to the mineralogy of Nova Scotia, 275.
- Hydrogen, on the spectra of, under high pressures, 366.
- Hypotheses, on the inexpediency of erroneous, 464.
- Ice, on the mechanical properties of, 1; on the veined structure of, 241; on the friction of iron upon, 467.
- Illumination in transparent substances, on, 221.
- Induction, on the laws of, 73.
- Iodine-vapour, on the absorption-spectrum of, 465.
- Iron, on the influence of sounding-vibrations on the magnetism of, 398.
- Jamin (M.) on the laws of induction, 73.
- Jargonium, on the new element, 65, 450.
- Jurassic and Cretaceous periods, on the limits of the, 321.
- Kirkwood (Prof. D.) on comets and meteors, 233; on the periods of certain meteoric rings, 394.
- Kohlrausch (Prof. F.) on the determination of the absolute horizontal intensity of the earth's magnetism, 114.
- Kolbe (Prof. H.) on the province of mineral chemistry, 348.
- Ladenburg (M.) on the action of sodium on acetic ether, 394.
- Lagrange, on an elementary proof of a theorem of, 107.
- Lead, on the solvent action of alcoholic saline solutions upon sulphate of, 154.
- Liquids, on the motions of certain, on the surface of water, 32.
- Lockyer (J. N.) on recent solar discoveries, 61.
- Lunar rays, on the calorific power of the, 78.
- Magnetism of the earth, on the determination of the absolute horizontal intensity of the, 114; on mural appearances and their connexion with the phenomena of the, 159.
- Magnus (Prof. G.) on the change in the radiation of heat by roughness of the surface, 445.
- Marie-Davy (H.) on the calorific power of the lunar rays, 78.
- Matter, on the continuity of the gaseous and liquid states of, 150.
- Mensbrugghe (Prof. van der) on the motions of certain liquids on the surface of water, 34.
- Meteoric rings, on the periods of certain, 394.
- Meteors, observations on, 233.
- Mica-combinations, on, 195.
- Mineralogy of Nova Scotia, contributions to the, 275.
- Moon, on the calorific power of the light of the, 78.
- Moon (R.) on the functions of the membranous labyrinth and semi-circular canals in the human ear, 248.
- Moseley (Canon) on the mechanical properties of ice, 1; on the veined structure of the ice of glaciers, 241.
- Müller (J.), physical notices by, 467.
- Natroboracalite, on the form and composition of, 275.
- Naumann (A.) on the deduction of Avogadro's law from the fundamental conception of the mechanical theory of gases, 317.
- Nebulæ, on a theory of, 401.
- Nitrogen, on the spectra of, under high pressures, 369.
- Norton (Prof. W. A.) on the fundamental principles of molecular physics, 126.
- Ocean-currents, on, 81, 180.
- Optical illusion, on an, 423.
- Optometer, on a new, 9.
- Oxygen, on the spectra of, under high pressures, 368.
- Parnell (J.) on secondary currents and a new secondary battery, 52.
- Phillips (J. A.) on the slates, felsites, and elvanites of Knockmahon, 12.
- Physical notices, 467.
- Physics, on the fundamental principles of molecular, 126.
- Pietet (F. J.) on the limits of the Jurassic and Cretaceous periods, 321.
- Polarization in transparent substances, on, 221.
- Proctor (R. A.) on certain drifting motions of the stars, 381.
- Radiation, on solar, 70, 299, 315.
- Rankine (W. J. M.) on the thermal energy of molecular vortices, 211;

- on the thermodynamic theory of waves, 306; on the mathematical theory of stream-lines, 460.
- Refraction-equivalents of the elements, on the, 231.
- Regnault (V.) on the expansion of gases, 127.
- Reusch (Prof.) on mica-combinations, 195.
- Roger (M.) on the laws of induction, 73.
- Roscoe (Prof. H. E.) on vanadium, 146.
- Royal Society, proceedings of the, 65, 146, 231, 304, 374, 450.
- Russell (W. H. L.) on the mechanical description of curves, 304.
- Schultz-Sellack (Dr.) on the diathermancy of a series of substances for obscure heat, 396.
- Secchi (Father) on recent solar discoveries, 61.
- Silliman (B.) on flame-temperatures in their relations to composition and luminosity, 290.
- Slates of Knockmahon in the county of Waterford, on the, 12.
- Solarradiation, researches on, 299, 315.
- Sorby (H. C.) on the new element jargonium, 65; on some remarkable spectra of compounds of zirconia and the oxides of uranium, 450.
- Soret (L.) on illumination and polarization in transparent substances, 221.
- Sound, on the structure of the human ear, and the mode in which it administers to the perception of, 248.
- Sounds, on the deadening of the, of solid bodies by internal resistances, 161.
- Spectra of gases under high pressures, on the, 365.
- Stars, on certain drifting motions of, 381.
- Stewart (B.) on auroral appearances and their connexion with the phenomena of terrestrial magnetism, 159.
- Stone (E. J.) on the heating-powers of Arcturus and  $\alpha$  Lyrae, 376.
- Storer (Prof. F. H.) on the simultaneous occurrence of a soluble lead-salt and free sulphuric acid in sherry wine, 154.
- Stream-lines, on the mathematical theory of, 460.
- Strutt (the Hon. J. W.) on an electro-magnetic experiment, 428.
- Sugar, on inverted, 345.
- Sun, on recent discoveries concerning the, 61.
- Templeton (R.) on Mr. Douglas's account of a new optometer, 9.
- Thalén (R.) on the absorption-spectrum of iodine-vapour, 465.
- Thermochemical researches, 410.
- Thermodynamics, on, 421.
- Thomsen's (J.) thermochemical researches, 410.
- Tides, a mathematical theory of the, 18, 260, 435; on some propositions in the theory of the, 49; on popular difficulties in the theory of the, 174.
- Tomlinson (C.) on the motions of certain liquids on the surface of water, 32.
- Tupper (J. L.) on an optical illusion, 423.
- Tyndall (Prof.) on the polarization of heat, 280.
- Uranium, on some remarkable spectra of compounds of zirconia and the oxides of, 450.
- Vanadium, researches on, 146.
- Vibration, on approach caused by, 309.
- Vortices, on the thermal energy of molecular, 211.
- Walenn (W. H.) on the atomic theory, 122.
- Wanklyn (J. A.) on the action of the alkali-metals on the ethers of the fatty acids, 286.
- Warburg (Dr. E.) on the deadening of the sounds of solid bodies by internal resistances, 161; on the influence of sounding-vibrations on the magnetism of iron, 398.
- Warren (T. T. P. Bruce) on Professor Fleeming Jenkin's formula, 169.
- Water, on the motions of certain liquids on the surface of, 32.
- Watts (Dr. W. M.) on the temperature and heating-powers of flames, 337.
- Waves, on the thermodynamic theory of, 306.
- Wine, on the occurrence of a soluble lead-salt and free sulphuric acid in sherry, 154.

Winter (G. K.), observations of the corona during the total eclipse of August 7, 1869, by, 17; on the determination of the size of the wire which, wound upon a galvanometer or electromagnet, will produce the maximum magnetic effect in a circuit, 109.

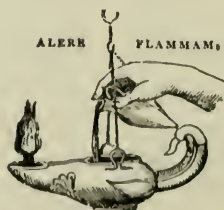
Wüllner (A) on the spectra of some gases under high pressures, 365.

Wurtz (H.) on flame-temperatures in their relations to composition and luminosity, 290.

Zirconia, on some remarkable spectra of the oxides of, 450.

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